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EDITORIAL.

When the AMERICAN JOURNAL OF PSYCHOLOGY was founded in 1887, it was a pioneer in its field. It represented the department of psychology at the Johns Hopkins University, was for years the only one of its kind in the country, and the establishment of which, as its subsequent history shows, was one of the boldest and most sagacious as well as one of the most successful and beneficent steps ever taken by this leader of the new academic movement. Here vigorous and creative minds like Cattell, Dewey, Jastrow, Donaldson, Cowles, C. L. Franklin, Hodge, Burnham, Patrick, Noyes, Nelson, Motora, Stevens, Edwards, and others, all of whom have enriched the department by original contributions in the JOURNAL, received, some a part and some all, of their special training. There was almost no outside aid, and for years practically no competition in any land or language. For years the struggle for existence was severe, and the editor himself did a good part of the review and other unsigned work, and made good the large annual deficit from his own pocket. Since moving to Worcester the JOURNAL has depended for original articles largely upon members of Clark University, men already prominent or promising in professonial position or productivity, like Franz Boas, A. F. Chamberlin, B. I. Gilman, B. C. Burt, Alfred Cook, C. A. Strong, A. MacDonald, Le Rossignol, W. L. Bryan, T. L. Bolton, Frederick Tracy, W. O. Krohn, Gerald M. West, A. H. Daniels, E. W. Scripture, Herbert Nichols, Alexander Fraser, F. B. Dresslar, John A. Bergström, Frank Drew, J. H. Leuba, H. T. Lukens, Colin A. Scott, G. W. A. Luckey,

G. E. Johnson. Under the influence of these men departments of experimental psychology and laboratories were founded at Harvard, Yale, Philadelphia, Columbia, Toronto, Wisconsin and many other higher institutions of learning; text-books have multiplied; other workers no less able who were trained abroad and elsewhere have come into the field; psycho-physics, neurology, the psychology of the insane, criminals, blind, deaf, idiotic and other defective classes, studies of animal instincts, anthropology and childhood, which the JOURNAL was the first to make it a leading purpose to bring out of their isolation into fruitful reciprocity,—have been well coördinated and other periodicals established. The JOURNAL invited and presided over the establishment of the American Psychological Association, which is the first organization in the country in which teachers of psychological and other philosophic subjects, who in the past have found it difficult to co-operate, have been united and felt some degree of *esprit de corps*.

It is now because recent developments enforce the necessity of defining and emphasizing anew a standpoint which is scientific, without mystic infiltration, unitary enough not to mix the most opposite tendencies in rococo confusion in the same journal and even text-books, and American in that instead of mere discipleship to past or present leaders, or excessive deference to European thinkers, it is clearly seen that our own country is in dying need of a science of man larger than any or all of the systems, that the AMERICAN JOURNAL OF PSYCHOLOGY, instead of lapsing, as it might well do after such service, to become the organ of one laboratory, takes up again its task, which is far harder even than before, but with greater resources and a settled conviction that the immediate future will see yet greater changes than the past, year for year, and with even greater courage and ardor than when it first started.

From the first the JOURNAL has slowly and steadily grown and now pays all its expenses, and with the present number begins changes and improvements along the following lines:

I. Several slight modifications in form.

II. A new department of notes, to which contributions are invited.

III. An international corps of editors and contributors, who will contribute and write reviews.

IV. An improved method of finding and obtaining IMPORTANT literature on the lines defined below, and of having it promptly noticed, digested or reviewed, so that our Review Department be not limited to what publishers choose to send.

Our suggestion to our reviewers is to make each review a miniature of the original, embodying its every new and salient phrase and idea, but boiled down to the utmost consistency, appending criticism, if any, in a final paragraph. This work is often so poorly done that the services it may render are but little known. These digests should be more *striking* than the original itself, where transitions are more gradual, the whole not seen in all its proportions at once. It is believed that publishers and readers alike will welcome this method.

V. Exclusion of all advertisements whatever, save occasional friendly exchanges with other scientific journals in cognate lines. By thus admitting between our covers only such titles among the vast mass of publications as we deem most worthy the attention of our readers, our reviews may perhaps be kept more impartial.

VI. A sharply defined field for both articles and reviews as follows:

I.

The results of experimental investigations in psycho-physic laboratories. To this *Archiv* function, not yet represented by any serial publication in this field in English, we are ready, if fit matter be forthcoming, to give most space, and should be glad to become the organ in which any American laboratory can be sure of publication in the order of its reception of any contribution of the methods or results of original research that is both new and important, and to print memoirs of greater length than has been possible hitherto, up to the utmost limits of our practical resources, provided, always, that there is the greatest practicable condensation and elimination of excessive discussion and unimportant details, to which young investigators in this field are so prone. Prominent German experimenters have illustrated that there

is such a thing as over-production of statistical tabulation on the one hand, and a use of exact apparatus in a way so lacking in rigor and severity as to positively embolden the speculative propensities so inveterate in this field, while in this country much of the psychology of the last decade is by "arm-chair professors," who lack patience for the tedious details of laboratory research as well as the instinct for concentration and specialization that can focus their efforts upon anything less than the entire field of psychology. This JOURNAL desires to represent neither of these tendencies, nor the disposition, now also too rife in this period of rapid transition, to press imperfectly established observation into the service of old discussions concerning problems not yet soluble by science, such as epistemology, the nature of consciousness, the freedom and essence of the will, the ego, immortality, etc., or idealism generally, on the one hand; or molecular tremors, phosphorescence, memory cells, chemical and electrical tropes by those who are neither chemists nor electricians, etc., or materialism generally, on the other hand.

II.

Studies in abnormal psychology, including the insane, criminals, idiotic, blind, deaf, or other defectives or degenerates. Here belong a large number of border-land phenomena not yet adequately represented in medical literature. Here premature conclusions, like the existence of a magnetic fluid, telepathy, spiritism, dream signs and prophecies, etc., which represent the largest number of articles thought by their authors to be psychological, but which the JOURNAL has had to decline, not purely because the bottom facts recorded were not of great interest and importance, but because the observation was utterly uncritical and distorted by crude superstition or crass theory on the one hand, and the Lombroso-Nordau tendency to find symptoms of disease or decadence in every exceptional trait or act, forgetting that the rough symptom groups found practical for the clinic are not the categories by which to diagnose the forces that make for higher human evolution and variation, on the other; — all this, from the standpoint of the JOURNAL, is far less scientific

than work in the method and spirit of Kandinsky, Krafft-Ebing, Kräpelin, Magnan, Cowles, and others. We should prefer to print studies like those the latter is now making at the palatial new asylum at Waverly, which marks a new departure in the systematic observation and treatment of the insane by combining and embodying in practical form the best new tendencies in psychology. The JOURNAL also desires to stimulate the scientific study of the feeble-minded, paupers and under-vitalized classes, as well as that of the blind and deaf,—to say nothing of freaks, cranks and other exceptional persons generally, and is fully persuaded that this field, now almost entirely uncultivated, will yield a fruitage no whit less valuable than that of the new criminology, if cultivated with equal vigor and sagacity. In this field psychology cannot experiment, but nature does so on a gigantic scale all about us, and we should now try to gather more of the lessons from her vast experiment station.

III.

The anthropology of myth, custom, religious belief, symbols, etc., among savages and ethnic stocks; rites, ceremonies and all products of the mythopœic faculty, and all expressions of the religious instinct are so spontaneous and central that it is strange that modern psychology has so ignored them. The grand old cult sometimes spoken of as conversion, the new life, regeneration, which in the old American college was central in all instruction in ethics, and mental science, as it has been in some form for adolescence in every religion, savage or civilized, can only be rescued from its present degeneration by such studies. The JOURNAL holds that anthropological is, to say the very least, no whit less important than physiological psychology as an element of philosophical training. The deep psychological significance of myth, rite, ceremonial, symbol, etc., are also just being discussed, and can be explored and utilized only by careful study and research in general anthropology, which is expected at least in one American university of all psychological students.

IV.

Closely connected with the latter is genetic psychology and exact and careful child-study by scientific observers. This movement is so recent, and by methods so new, that American psychologists have little conception of its scope. Not only is it repeating, stage by stage, the history of the laboratory movement, but marking, as it does, the first advent at last of evolution in the study of the soul, it promises to equal the latter in importance, and relegate much of the present adult psychology to those pages of history which preserve the aberration and over-subtleties of vigorous but misdirected minds. The JOURNAL can only print the most exact and scientifically important researches in this field. For those of a more popular and practical nature, another journal has been especially established *(The Pedagogical Seminary)*.

V.

Studies of animal psychology.

VI.

Neurological researches.

VII.

The psychology of philosophy, ethics, æsthetics, theology, etc. To the psychological treatment of subjects or persons in this field by the same objective and critical methods, as myth, art products, or other more naïve creations of the soul, are discussed from a more conscious and scientific standpoint, as the psychology of genius, childhod, insanity, instinct are studied, great importance is attached.

Thus the field of the JOURNAL does not include the history of philosophy, ethics, or pedagogy, for these topics are already adequately provided for in other journals. It does not include epistemology, metaphysics, sociology, or theology unless treated as above, nor offer to print long discussions concerning matters on which conclusions are impossible. The JOURNAL thus has a philosophical *standpoint* and *character*.

EXPERIMENTS ON FECHNER'S PARADOXON.

By T. R. Robinson, B. A., Toronto.

Psychology, at least at an earlier period of its history, has had to defend its claim to be considered as an exact science. The best defense of this claim consists in showing that not only are many of its problems insoluble for physics and physiology, but that for these sciences they are not problems at all. The first of the general questions of experimental psychology is that of the quantitative relation between an external excitation and the corresponding internal reaction or sensation. One of the most interesting phases of this question concerns those cases where the constituting parts of the stimulus are applied to different though coördinate sensitive surfaces, *e. g.*, in the case of the organs of sight. The present article deals with the relation of the light intensity of an object seen with both eyes to that of the same object seen with only one. Its purpose is to give a brief account (1) of the work previously done upon this problem, (2) of the writer's own work upon it.

I.

The first investigation of this problem was undertaken nearly a century and a half ago by Jurin, who found by experiment that an object appears measurably brighter regarded with both eyes than with only one. His method may be schematically illustrated by Fig. 1.

A sheet of white paper, P, was illuminated by two candles, L_1 and L_2, placed behind it. A screen, S_1, was interposed in such a way that the right half of the paper received the light of both candles, the left half only the light of one. A

second screen, S_2, was placed before the right eye of the observer in such a way as to hide from it the brighter half of the paper. The left, or less illumined half, was now seen by both eyes, and the right, or brighter half, only by the left eye. It was found that the left, or darker half, seen with both eyes, appeared about equally bright with the right, or brighter half, seen with the left eye, when the one light was about 3.4 times as far distant as the other, so that the intensities of the brighter and the darker halves bore to each other the relation

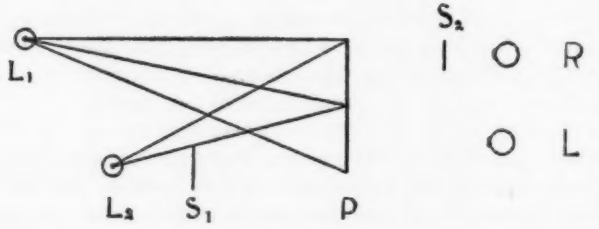


FIG. 1.

of 13 to 12. According to this result, the same object, or one equally bright, would appear in binocular vision $\frac{1}{13}$ brighter than in monocular vision.

The problem was dealt with by a somewhat more accurate method by H. H. Valerius in 1873, by means of an application of Faucault's photometer. This photometer consists of a box, the interior of which is lined with black cloth to prevent the reflection of light rays. In one end of the box is a semi-transparent glass disc, placed so as to admit the lights whose intensities are to be compared. The box contains a sliding diaphragm, which, by means of a screw, can be placed nearer to or further from the disc. The lights to be compared are now placed one on each side of the diaphragm, in such a way that by adjusting the distance of the diaphragm from the disc, each light illumines exactly one-half of the disc. The observer, looking from the outside at the disc, adjusts the distances of the two lights from the halves of the disc which they respectively illuminate, so that the whole surface of the disc appears equally bright.

Then these distances are measured and the relative intensities of the light determined by the rule that they will be inversely as the squares of their distances from the illuminated object. Since Valerius has not illustrated his article by diagrams, it may be worth while to attempt a schematic representation of his arrangement in Fig. 2.

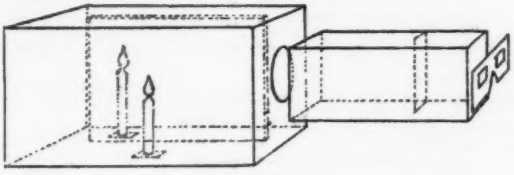


FIG. 2.

The mode of using this apparatus in the experiments of Valerius was as follows: The two lights were introduced as though their intensities were to be compared and adjusted, so that the whole surface of the disc was equally illuminated and their distances noted. The observer looks through the tube, keeping the position of the head constant by means of a screen, with openings for the eyes and a slit for the nose. In the interior of the tube is a diaphragm, which conceals one of the vertical halves of the disc from one of the eyes of the observer. The result is that one of the vertical halves of the disc is seen with both eyes, the other with only one. When this is the case, the half seen only with one eye appears less bright than the other. This is remedied by moving the light which illumines the former nearer to the disc, until the two halves again appear equally bright. This new distance is also measured and compared with the former distance of the same light. Now, if we denote by I the brightness of the half of the disc under consideration when the light is at the first distance d, and by J the brightness when the light is at the lesser distance d', there is between J and I the relation $J:I::d^2:\ :d'^2$, and, consequently, since the intensity I of the light seen with both eyes is equal to the intensity J of the same light seen with one eye, we have as an expression of the relation of the light intensities of binocular and

monocular vision the ratio $d^2 : d'^2$. The following is Valerius' statement of his experiments and their results :

FIRST SERIES, MADE WITH THE FLAMES OF TWO CANDLES.

Distance of Right Candle from Right Half of Disc.	Second Distance.	Relation of the Two-Light Intensities.
100 Ctm.	94 Ctm.	1.15
75 "	71 "	1.11
62 "	53 "	1.14
41 "	38 "	1.16

SECOND SERIES MADE (BY ANOTHER OBSERVER) WITH TWO GAS FLAMES.

Distance of Right-Hand Flame from Right Half of Disc.	Second Distance.	Relation of the Two-Light Intensities.
100 Ctm.	94 Ctm.	1.13
75 "	71 "	1.11
62 "	57.5 "	1.16
41 "	47.5 "	1.18

From these results Valerius draws the following conclusions : 1. The relation of the light intensities of the same object, observed successively with one eye and with two, appears to be almost entirely independent of the absolute intensity. 2. For weak lights, such as those of the ordinary candle or gas flame, this relation does not vary much from 1.15.

These early experiments, though scientific in principle, are defective in several respects.

1. The methods of both Jurin and Valerius are open to the objection, which Valerius afterwards noticed, that the sensitiveness to light of the two eyes of the same individual is commonly not the same, and this may materially affect the result.

2. The two eyes were not, in the experiments, subjected to the same treatment. One eye received continuously more light than the other.

3. Though the object observed was screened from one eye, much light was still admitted to that eye, a fact that would doubtless have an influence on the intensity of the whole.

4. The trials were not sufficiently numerous or varied to

warrant the conclusion of Valerius that the relation does not depend on the absolute intensity, nor does that conclusion seem to be borne out by the trials that were made; for the relation seems to vary with the absolute intensity, though there is not much constancy in the results. Another consideration overlooked by Valerius was that it is only possible for the absolute intensity to affect the relation if the first impression both of the one eye and the two are taken, for after the observer has looked for some time at the object, the eyes become adapted to the absolute intensity, so that it can no longer affect the relation.

5. It would appear that both Valerius and Jurin fix the relation too exactly, because they take no account of the subjective conditions on which the results of their experiments must in large measure depend; for we are not comparing absolute light intensities, but only the intensity of light sensations.

Fechner made, in 1860, some experiments at Leipsic, from which it appeared that with most observers the closing of one eye caused a slight darkening of the whole visual field, followed immediately, however, by a restoration of its brightness, whence he concluded that the intensities of monocular and binocular vision are equal. Aubert, however, following the method of Fechner, found that the light intensity of the whole visual field was somewhat greater when both eyes were open than when one was closed, provided that the absolute intensity were not greater than that of white paper in diffused daylight. These experiments do not, however, possess much value for the solution of our problem (viz., to find how much the intensity of monocular vision is increased by the addition of the other eye), because they seem to have been made with reference to continued observation both in binocular and monocular vision, where the one eye, becoming accustomed to working alone, is not in the same condition as if the first impressions had been taken.

In the course of further trials, under different conditions, however, Fechner found: (1) That when the visual field of one eye is darkened by means of a smoked glass, and then the common visual field, or a white object in the common

visual field, is regarded, the latter appears darker than if the eye partially obscured by the glass is closed. (This, Fechner calls the "paradox trial," because the total darkening of one retina causes a brightening of the whole visual field.) (2) That an equal darkening of the common visual field results from placing before one eye a glass which absorbs very little or one which absorbs very much light. This equal darkening of the whole visual field, by unequal components, Fechner calls the conjugate intensities. With a certain light absorption occurs the maximal darkening of the whole vision; this point Fechner calls the minimum point.

In these experiments the darkening continued for some seconds, so that its extent could be estimated. But if the glass before one eye were very dark, and the observer continued to look, for say a minute, there occurred an alternate darkening and brightening, the so-called competition phenomenon of the visual fields. For this reason Hering regards Fechner's trials solely as instances of the competition phenomenon. Helmholtz, on the other hand, holds that in these trials we have not a change in the sensation of brightness, but only a change in our judgment regarding the surface-color of the white object. Aubert rejects both these views as inconsequential, though he admits that the use of an object with strongly marked lines or contours has naturally a disturbing effect upon the simplicity of the light sensation. According to Aubert the trials show that a combination of the sensations of the two retinas occurs when the difference of their intensities does not go beyond a certain point, which the experiments themselves must determine, but beyond this point the capability of combination decreases and finally ceases altogether. He thinks, also, that the absolute intensity of the object affects the possibility of combination.

In the similar experiments made by Aubert himself, a double episkotister was used, having fixed before it a screen with openings for the two eyes of the observer. One disc corresponds to the lighter smoked glass of Fechner, the other to the darker. The episkotister has the advantage of giving an exact determination and variation of the intensity,

and also of furnishing an absolutely colorless grey, while the smoked glasses have almost always a certain color, which makes it extremely difficult to compare their intensities. The greatest darkening in the common visual field occurred in Aubert's experiments, when one eye was free and $\frac{122}{1000}$ of the full light was admitted to the other; *i. e.*, if the intensity of the full light = 1,000 when a light of the intensity of 122 is admitted to one eye while the other is unobscured. On the admission of less light, the common visual field appeared brighter, and the same result followed on the admission of more light. There must, therefore, be found total intensities which are equal to each other when one eye looks through a disc, which admits say 55 parts of light, or through one which admits say 500. Fechner represents these numbers on a curve, the shortest ordinate of which corresponds to the greatest darkening in the common visual field; the lowest resulting point of the curve he calls the minimum-point, the equal intensities in the common visual field upon the greater and less darkening of the one eye the conjugate points of the curve. This mode of representation, with the modification required by the slightly different results of Aubert, may here be reproduced in Fig. 3.

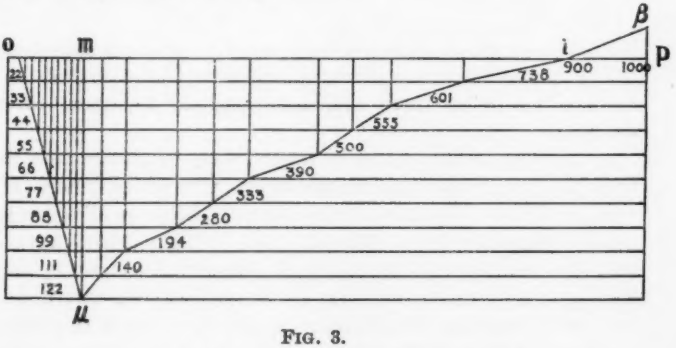


FIG. 3.

op. represents the intensity of the light sensation of the whole visual field when one eye is closed. The point β of the ordinate above *op.* represents the somewhat greater intensity when both eyes are open, which, according to Aubert,

makes a difference of about $\frac{1}{10}$, rather more than Valerius or Jurin found it; μ, the lowest point of the curve, represents the minimum-point, which corresponds to the sensation of least light in the common visual field when one eye is unobscured. This point was reached when for the other eye 0.122 of the full light was admitted by the episkotister, and the darkening of the whole visual field was then as great as when with monocular vision 0.583 of the full light was admitted. These numbers Aubert found to be somewhat different when, instead of a sheet of white paper in diffused daylight, he took as objects, successively, the sky, the white glass shade of a lamp and the free lamp flame. His results concerning the conjugate intensities may be given in the following table corresponding to Fig. 3.

Paper.	White Glass Shade.	Sky.	Free Flame.
22 = 738	16 = 750	16 = 700	16 = 444
33 = 601	22 = 666	22 = 500	22 = 377
44 = 555	33 = 400	33 = 333	33 = 333
66 = 390	44 = 333	44 = 128	44 = 250
77 = 333	55 = 250	55 = 83	55 = 200
88 = 280	66 = 166	66	(66 - - - 166)
99 = 194	(77 - - - 140)	(333)	
111 = 140			
122 = (417)			

In explanation of the general phenomena of the coöperation of the two eyes, there are, according to Fechner, three theories.

1. The combination theory, according to which the total intensity equals the sum of the monocular intensities, where this sum is subject, of course, to the same condition as all summation of intensities (*i. e.*, Weber's law). This theory agrees with the fact that the intensity of binocular vision is not double that of monocular, but it does not explain why under certain circumstances a decrease of physical intensity causes an increase of intensity in sensation.

2. Theory of attention. According to this theory, in the case of smaller differences of the impressions, the attention is distributed upon both the impressions, while in the case of greater differences the attention is directed exclusively to the

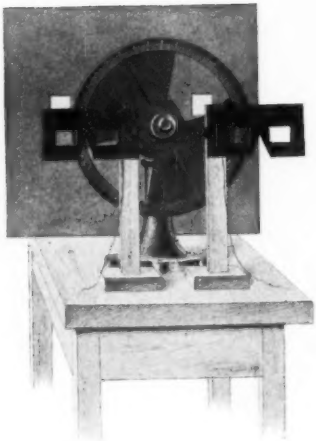


FIG. 4.

brighter retinal image. This is the theory which seems to be favored by Aubert. It may be objected that the same thing should hold good in the different parts of one retinal image, where all parts are not equally bright, *i. e.*, the brighter part should monopolize the attention to the exclusion of the other. Further, it is an error to speak of the two retinal images as if they existed separately in consciousness. In our perception there exists only *one* visual field. That we regard *two* similar images in this visual field sometimes as belonging to two similar objects, and at other times as double images of the same object, does not depend on the intensities of these images.

3. Theory of antagonism. This is Fechner's own theory. It explains the phenomenon as coming under the general phenomenon of competition of the visual fields. According to this view, the impressions of the two eyes are combined when the difference of intensity and quality are not very great, while in the case of greater differences no combination takes place, but either the one of the images (generally the less bright) is suppressed entirely, or the two images replace each other alternately.

The above is a short résumé, so far as the literature of the subject was obtainable by me. The following are the references: Valerius, Poggendorff's Annalen, Band CL, p. 317; Jurin, Smith-Kästner, Lehrbegriff der Optik, 1755, p. 479 (Jurin's work is also reported by Aubert); Fechner, Binoculares Sehen, in Abhandlungen der Akadamie in Leipzig, 1860, Band VII, p. 423; Aubert, Physiologische Optik, p. 499, and Physiologie der Netzhaut; Helmholtz, Optique Physiologique, 1st ed. p. 964; Wundt, Physiologische Psychologie, Vol. II, 4th ed., p. 210 ff.

I shall now add an account of some experiments made during the current year in the psychological laboratory of University of Toronto, under the supervision of Dr. Kirschmann.

II

The apparatus employed in this work was a single episkotister turned by an electric motor, as shown in the accompanying cut, Fig. 4.

Behind the episkotister were placed the objects to be observed. In a large sheet of black card-board two square holes, $3\frac{1}{2}$ ctm. square, were cut and covered with white tissue paper, and behind each opening was placed alternately an incandescent lamp of 32-candle power. The intensity of the light was varied by using more or fewer sheets of tissue paper. The episkotister was graduated in 360°, and was arranged so as to vary the light admitted between the limits of 0° and one-half the total intensity (=180°). The illuminated squares were placed in line with the edges of the disc, one on the right hand, the other on the left. In front of the disc were two screens with openings for the two eyes of the observer, and slits for the nose in order to keep the head steady. These screens were placed so that through one of them the right hand light was seen, through the other the left, and were also arranged in such a way that by means of them one eye saw the light through the episkotister, the other looked directly at it. The small shutters shown in the cut were used to cover the eye, for which the light was partially obscured by the episkotister. During the experiment all other light than that of the electric lamp in use in the experiment, was carefully excluded from the room. In the use of the *two* objects and *two* screens there was a double purpose: (1.) To avoid possible errors due to a difference between the two eyes of the observer. (2.) To subject both eyes throughout the experiments to the same treatment, and so to avoid another source of error.

Before describing the method adopted in the experiments, it is necessary to more clearly define their object. There are two questions which do not seem to have been clearly distinguished by former investigators: (1.) The question, to what extent an object appears brighter or darker accordingly as it is continuously regarded under similar conditions with two eyes or with one. Here we have to do with a continuous state in coöperation or non-coöperation. (2.) The question, how much intensity of light sensation is added to that of monocular vision by the addition of the other eye, or subtracted from that of binocular vision by the closing of one eye? Here we deal with the immediate effect of a change. Viewing the

problem from the first standpoint, we have to seek for an equation between binocular and monocular intensities. From the other standpoint the problem presents itself as follows: For every intensity in monocular vision there exists a certain other intensity, the admission or non-admission of which to the other eye has *no effect* on the total intensity. To find for some cases these physical intensities, which, as far as it concerns the intensity of light sensation, are entirely ineffective, is the purpose of our experiments.

Fechner's paradox trial had shown that if one eye were partially obscured by a smoked glass or similar means, there occurred a brightening of the whole vision field when that eye was closed. It appeared, however, from some preliminary trials, that this is only true if a glass is used which absorbs most of the light. On the other hand, if a glass or episkotister is used, which absorbs comparatively little light, on the closing of the one eye the whole visual field appears darker. Between these limits, therefore, there must be, corresponding to Aubert's and Fechner's minimum-point, an indifference point, where no difference will appear in the intensity of the common visual field, or of an object in the common visual field on the closing of the one eye. To find this point, then, was the object of these experiments.

Placing himself before the left-hand screen of our apparatus with his eyes to the openings, the observer looks at the white square with the left eye free and the right eye darkened by the episkotister, admitting only a few degrees of light. After looking for a moment he pulls the string attached to the slide, thus shutting off the object entirely from the right eye, and immediately reports whether the object looks more or less or equally bright. Then changing over to the right-hand screen, he repeats the trial, having now the right eye free and the left partially obscured. Then the episkotister is readjusted so as to admit a little more light and the trials made again, beginning this time on the right side and changing over to the left, and so on through all the degrees of light between the two extremes. It was usually found that the indifference point did not occur upon the admission of one particular degree of light, but usually extended over a considerable

number of degrees, and that often when the object had begun to appear darker or brighter, it would, upon a further change, again seem equal. At the conclusion of a series of trials the average of the equal points was taken as representing the indifference point for that series. And where, as sometimes happened, there was a sudden change from brighter to darker, a point midway between was taken as the "equal" point. All the trials were made under similar conditions by two observers. In order to vary the conditions as much as possible, one series was made beginning with the episkotister admitting 5° of light and proceeding upwards to 180°, the next proceeding from 180° to 5°, the next beginning within the limits of the "equal" points and proceeding both up and down till those limits were passed, and then going back again to the region of equality. Different absolute intensities were used and a series of trials made for each, the intensity being varied, as already said, by placing more or fewer sheets of tissue paper over the apertures. There was found to be a variation in the results in close correspondence with the variations in the absolute intensity, as shown in the accompanying table. Some supplementary trials were also made with pure colors, the results of which are also appended. In the table the absolute intensity used in the first series of trials (that of a 32, candle power lamp behind two sheets of tissue paper) is taken as 360°, and the others in comparison with it, and measured by means of an episkotister photometer.[1]

In the case of the observer K the results for the two sides were so different that they had to be given separately. The two eyes of the observer K, although in the same state refractively, are in several respects considerably different. The left eye has an iris of different color and a considerably smaller pupil than the right. With the other observer,

[1] If one tissue paper allows $(\frac{1}{n})$ of the incident light to pass, then through 2 papers $(\frac{1}{n})^2$ should be transmitted, through m papers, $(\frac{1}{n})^m$, according to the theory. By photometrical measurement the transmission through several sheets is always found to be a little greater than the computed value. This is due to the circumstance that by the contact of the different sheets the number of absorbing and diffuse-reflecting surfaces is diminished.

the combined results of the two eyes are given, though in this case, also, a difference was noticeable, though smaller and less constant.

TABLE I.

EXPERIMENTS WITH WHITE LIGHT.

Number of Tissue Papers.	Photometrically Determined Intensity.	OBSERVER: K.						OBSERVER: R.		
		LEFT SIDE.			RIGHT SIDE.			LEFT AND RIGHT SIDE.		
		Av. Value.	m.V.	Ratio of the Full Light.	Av. Value.	m.V.	Ratio of the Full Light.	Av. Value.	m.V.	Ratio of the Full Light.
2	360	$52\frac{35}{72}°$	$4\frac{1}{72}°$	0,146	$67\frac{2}{3}°$	$1\frac{1}{15}°$	0,188	$63\frac{257}{312}°$	$7\frac{145}{312}°$	0,177
4	210	$62\frac{47}{72}°$	$\frac{17}{72}°$	0,175	$73\frac{11}{36}°$	$2\frac{77}{115}°$	0,203	$77\frac{227}{1220}°$	$2\frac{671}{1220}°$	0,214
6	120	$67\frac{12}{72}°$	$9\frac{1}{72}°$	0,188	$76\frac{2}{3}°$	0°	0,213	$99\frac{11}{16}°$	$5\frac{5}{16}°$	0,277
10	12(?)	$77\frac{6}{7}°$	$\frac{1}{7}°$	0,216	$106\frac{12}{16}°$	$\frac{35}{36}°$	0,296	$121\frac{19}{32}°$	$4\frac{18}{32}°$	0,337
10 and 2 sheets of ord'y white paper.	1	$127\frac{1}{2}°$		0,354	$132\frac{1}{2}°$		0,368	165°	5°	0,458

TABLE II.

EXPERIMENTS WITH COLORED LIGHT.

Color.	OBSERVER: K.						OBSERVER: R.		
	LEFT SIDE.			RIGHT SIDE.			LEFT AND RIGHT SIDE.		
	Av. Value.	m.V.	Ratio to the Full Light.	Av. Value.	m.V.	Ratio to the Full Light.	Av. Value.	m.V.	Ratio to the Full Light.
Red	$83\frac{1}{2}°$	$3\frac{1}{8}°$	0,231	$98\frac{11}{18}°$	$3\frac{11}{18}°$	0,274	$110\frac{5}{12}°$	5°	0,307
Green	$100\frac{5}{8}°$		0,280	$123\frac{1}{3}°$		0,343	$92\frac{9}{72}°$	$\frac{47}{72}°$	0,256
Blue	$95\frac{5}{17}°$	$5\frac{5}{17}°$	0,265	$106\frac{12}{22}°$	$3\frac{9}{22}°$	0,296	$118\frac{1}{2}°$	$1\frac{1}{2}°$	0,329

In Table I the numbers given as "average-values" represent the number of degrees of the episkotister, through which the light had to pass in order to produce no effect on the total intensity. These numbers are attained by averaging, in the case of observer K, the results of two double series of experiments; in the case of observer R, of four double series.

The second table contains the results of a few experiments with colored light. In these experiments the two openings

which served as objects were covered with a combination of tissue papers and colored gelatine plates. Three combinations of apparently equal brightness were selected with the help of the spectroscope. The one permitted the transmission of the red end of the spectrum only, up to the line D, while the second absorbed all light at the ends of the spectrum, allowing only the transmission of the rays between D and F, and the last combination extinguished all rays less refrangible than F. We found the judgment in the case of colored light more difficult and uncertain; the region of equality is distributed over a larger field. There is a remarkable difference between the two observers. For K the greatest average value is found in green, where R has the smallest. If differences in the intensity of our colors, which could not be entirely excluded, were the cause, we should expect another result. If our green was brighter than the two other colors, it should have the smallest average value for both observers. But on the other hand it is quite possible that the same color has different values of intensity for different observers.

By *m. V.* we denote in our tables the mean variation, *i. e.*, the difference between the result of the single series and their average. Where the place for the mean variation is left empty in the tables, the results refer to one series of experiments only.

If we now cast a glance at our tables in order to form an opinion on the bearing of our results, we notice that they differ in two points considerably from those of former investigation. *First*, the minimum point of effectiveness of the light applied to the second eye (or in the terms of Fechner and Aubert, the maximal point of obscuration of the common visual field) is found at higher intensities than by former authors on the subject. *Second*, the phenomenon is greatly dependent on the absolute intensity.

Concerning the first point, it is true we have to assume that we should arrive at lower values for the minimum point, if we should proceed to higher intensities than 360°. We should expect that there is an intensity for which the average value of the point of ineffectiveness would show the ratio 0,122 of the full light, as found by Aubert. Of greater importance is

the second point, the dependence of the phenomenon on the absolute intensity. This dependence presents itself in our table I in such an obvious and regular manner that it is astonishing that it could escape the notice of former investigators. But we must not forget that they worked under entirely different conditions. Also the difference in the results of observer K, for left and right side, may have its cause in the different sensitiveness to light of the two eyes.

Remark on the Foregoing Article.

By A. Kirschmann.

The above reported experiments do not claim to be decisive in so far as concerns the absolute values of the minimal point of efficiency, and it is less the intention of the article to solve the problem definitely than to direct attention again to this subject, which touches so many questions of interest in the psychology of the sense of sight. However, this much may be concluded with certainty from these experiments, namely, that the phenomenon referred to is dependent on the absolute intensity. For small absolute brightness the loss of intensity in binocular vision is comparatively greater than for higher intensities; or in other words, the ratio of apparent intensities of an impression in monocular and binocular vision cannot be considered as constant. A few remarks will perhaps contribute something to the explanation of the paradox trial in particular, and of the problem of the cooperation of the two eyes in general.

What is the paradox in Fechner's experiment? That a decrease of physical intensity is followed by an increase of intensity in sensation. Or, in our special case, that a certain amount of physical intensity, applied to the one retina, has no effect on the total brightness of the binocular impression. But it does not follow from this that it has *no effect at all.* Its effect goes in another direction. The double eye has not the purpose of increasing the total intensity. Its principal function is to accomplish those parallactic relations which serve as the chief means of depth-perception. If to the

image of one eye that of the other is added, the result is something else than a mere summation of intensities. A part of the physical energy which now reaches the two retinæ will be used to accomplish the new result, the creation of a single image and the projection of it into the third dimension. Now since these parallactic relations, which give rise to our depth-perception, are independent of intensity, the energy needed to produce these effects will not be proportionate to the total energy, but it will in all cases demand a certain amount, below which the effect will not be attained.

Let us call the physical intensity which arrives at the one retina i_1, that arriving at the other i_2, and that physical energy which is at least necessary in order to produce the binocular effect x. Now there are three cases possible. If i_1 and i_2 are both greater than x, in the case of binocular combination, the subtraction of the energy, x, which is needed for this effect, will cause a darkening of the binocular visual field, but the brightness will still be greater than either i_1 or i singly. If x is just equal to one of the monocular intensities, the binocular intensity will be equal to the other monocular. The closing of the eye, in which the image had an intensity equal to x, will then cause the vanishing of the binocular space-effect, but without any change in intensity. This is the case where we have just reached the point of inefficiency. Finally, if one of the monocular intensities, say i_2, is smaller than the minimum value of x, the intensity of the binocular impression, when endowed with three-dimensional properties, will be smaller than i_1, because a part of this physical intensity is needed in order to secure the stereoscopic effect, and the exclusion of the second eye will, by setting free again this part of the energy, be followed by an increase of the light intensity. This is the case in Fechner's paradox experiment. This theory would account quite well for the paradox experiment, but it does not for the conjugate intensities. In order to explain this side of the affair, we have to make the additional assumption that in cases where one of the monocular intensities is very small, while the other is comparatively high, the binocular effect is incomplete or vanishes entirely. The works of previous authors are not quite

clear on this point, but it seems to me quite natural that, if to the one eye is applied the intensity 1, to the other the intensity 0,122, or less than that, the stereoscopic effect is lowered or even excluded. After all it is not necessary that the maximal point of obscuration coincide with our point of least effect on the total of visual field. According to the foregoing remarks, it remains, therefore, a problem of further investigation whether or not the paradox phenomenon takes place equally in the case of real binocular combination with three dimensional properties, and in cases of partly co-inciding double-images.

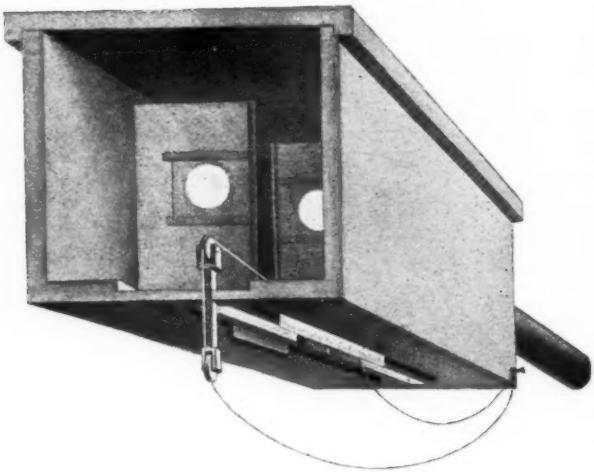
THE INFLUENCE OF THE COLOR OF SURFACES ON OUR ESTIMATION OF THEIR MAGNITUDE.

By J. O. Quantz, B. A.

One of the most generally known optical illusions is the apparently increased diameter of the rising moon. For this phenomenon there are different explanations. It is said that if the moon be near the horizon, we have the opportunity of comparing it with objects at a distance, but well known to us. When the moon is high in the sky, near or at the zenith, —where the diameter is really a little greater, on account of its being nearly 4,000 miles nearer,—we have no such opportunity. This explanation, however, is not satisfactory; for if we see the high-standing moon re-appear behind steep rocks or high buildings, the phenomenon does not occur.

A more satisfactory explanation is this: We project the celestial bodies on the surface of the sky. But the apparent form of the sky is not that of a sphere, but of a flatter vault, like a watch-case. Now, we attribute to all objects which we project on a surface thesize which the corresponding part of the surface itself would have at the distance at which we think we see it.

Whatever the true explanation may be, there is one point which has been overlooked. When the moon appears very large at the horizon, she has always a strongly orange or reddish color. The same is the case with the rising and setting sun; and it can really be observed that the phenomenon is less conspicuous when the reddish color is absent. This, however, does not prove that the redness of the moon is one of the causes of the illusion. The two phenomena may perhaps be traced back to the same cause. It may be, for example, that the moisture of the atmosphere, which causes



the red color by its absorption of the more refrangible rays of light, is also, on account of the dimness in which distant objects appear through it, the cause of our illusion. But it remains a question of interest whether this red color has anything to do with the geometrical optical illusion. From this particular phenomenon arises a more general question: Has the quality of light sensations an influence on the estimation of size? With a view to the solution of this problem, the experiments reported in the following pages were undertaken.

The apparatus used, of which the accompanying figure gives an idea, consisted of a large case, having one end open, and at the other an observing-tube. The case was painted on the inside a dead black to prevent its reflecting light; the tube was covered inside and outside with black velvet. Within this case were two movable black screens in a plane at right angles to the line of vision. Each of these screens was provided with an opening, in order to receive the objects. The latter were diaphragms of thin brass with a circular aperture, behind which were placed gelatine papers of different colors. In order to prevent the light from passing between the screens, the latter were provided at their inside edge with strips of black velvet, which overlapped each other without hampering the movement.

In a few experiments where two white circles were compared, the circles were equal, but for the others unequal; so that when they appeared to be of the same magnitude, they would not be at the same distance from the observer's eye, *i. e.*, not in the same plane. This excluded the possibility of judging the discs to be equal by noticing that they were in the same plane. The right hand diaphragm was stationary, at a distance of 1,240 mm. from the eye of the observer; the left movable, running in a slit in the bottom of the case. This movement, in both directions, was made by means of pulleys at the open end of the case, remote from the observer, and cords attached to both ends of the frame supporting the diaphragm which contained the disc; so that the observer himself was able to regulate the distance. Outside and underneath the case, to admit of being seen without removing the lid, was a millimeter scale, zero being in the plane of the

TABLE I.

Apparent Color.	Composed of		Visible Part of the Spectrum.	Remarks.
	Colored Gelatine Films.	White Tissue Papers.		
Red,	1 purplish red, 1 orange,	1	Red end to 598 $\mu\mu$,	
Orange,	1 rose, 2 orange,	1	696 $\mu\mu$ — 580 $\mu\mu$,	Also weak traces of green near 515 $\mu\mu$.
Yellow,	4 yellow,	1	Red end —509 $\mu\mu$,	Near red end very weak.
Green,	2 green,	1	566 $\mu\mu$ — 512 $\mu\mu$,	
Blue-green,	2 blue, 1 green,	1	556 $\mu\mu$ — 434 $\mu\mu$,	
Blue,	2 blue,	1	536 $\mu\mu$ to the violet end,	
Violet,	2 (different) violet,	1	Whole spectrum visible, with exception of 614 $\mu\mu$ —539 $\mu\mu$,	
Purple,	2 violet, 1 magenta,	1	Red end—600 $\mu\mu$ and 462 $\mu\mu$ —violet end.	Violet end very faint.

fixed disc. The perpendicular part of the sled of the movable object, reaching down a little deeper than the scale, had an arrow-formed white mark, which served as an index. The whole apparatus was placed with the open side against a window with white ground-glass. Fig. 1 shows the arrangement seen from the open side. Each trial consisted simply in changing the position of the left disc to the point at which it *appeared* to be equal in magnitude to the right, and then recording its position. Each color was compared separately with white. A series of trials was made with two white discs to determine what variation might result, which was not due to color.

The quality of the colors used, according to a spectroscopical examination, is shown on the opposite page. The wave-lengths are obtained by graphical interpolation.

The method of calculation may be outlined thus: Figs. 2 and 3 show a horizontal section, Fig. 2 having the larger

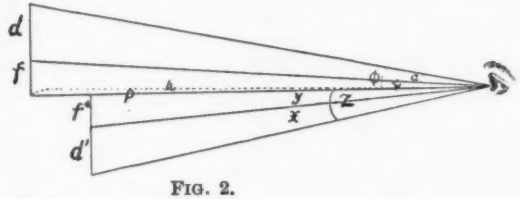


FIG. 2.

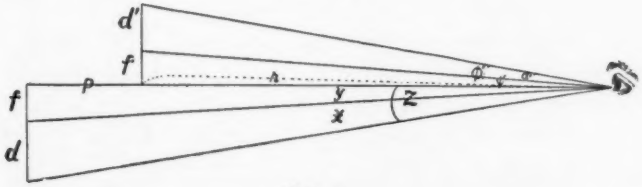


FIG. 3.

circle in the fixed position, and Fig. 3 the smaller. d is the diameter of the larger circle (40.575 mm.); d' of smaller (36.703 mm.), which is therefore nearer the eye when they appear of equal magnitude; f half distance (19.5 mm.) between the discs; r distance of the eye (1.240 mm.) from the position of the normal disc (stationary at right side); ^{a},

visual angle of larger disc in normal position ; a', of smaller ; ψ, visual angle of half distance between discs at normal position ; $\phi = a + \psi$, $\phi' = a' + \psi$.

a (easily found by trigonometrical calculation by means of the formulæ :

$$\tan\phi = \frac{f+d}{r}$$

$$\tan\psi = \frac{f}{r}$$

$$a = \phi - \psi\ )$$

For the colors orange, blue-green, violet and purple :

$$a = 1^\circ\,52'\,21''.8$$

Similarly : $a'\ (\phi' - \psi\ ) = 1^\circ\,41'\,39''.2$

The visual angles of the moved object and also the angular value of the mean variation were determined by means of the same formula. If we denote by ρ that distance of the movable object, in which it appears of equal size with the normal disc, by y and z the angular distances of the inner and outer edge of the movable disc from the median line, and finally by x the visual angle of the movable disc, then we have

$$\tan z = \frac{f+d'}{\rho} \text{ resp. } \frac{f+d}{\rho}$$

$$\tan y = \frac{f}{\rho}$$

$$\text{and } x = z - y$$

The angular value of the mean variation has been determined by the following procedure. Suppose the average of the registered values of a series of 50 or 100 single trials (ρ_1, ρ_2, $\rho_3 \ldots \rho_n$) to be ρ_m and the mean variation Δ. Now we compute the visual angle (x) for :

$$\rho = \rho_m$$

$$\rho = \rho_m + \Delta$$

$$\text{and } \rho = \rho_m - \Delta$$

Then the angular value of the mean variation will be

$$\frac{[x\ (\text{for}\,\rho_m + \Delta) - x(\text{for}\,\rho_m)\ ] + [x(\text{for}\,\rho_m) - x(\text{for}\,\rho_m - \Delta)\ ]}{2}$$

An equal number of trials was made with each eye. Each series of experiments was computed separately, and afterwards combined in tables. The series numbered I to IV under "Division of Experiments," in the subjoined tables, were arranged as follows: In I the colored circle was the

larger, and to the right, *i. e.*, stationary; II, right, and small; III, left, large; IV, left, small. Fig. 2 above thus represents the series I and IV, while Fig. 3 shows the position for II and III. For the colors, red, yellow, blue and green, there were only two divisions corresponding to IV and III respectively, the colored disc being always at the left. The experiments with these colors were made first, in 1893-94; all others were done in 1894-95.

For each series the average distance of the movable (left hand) disc was found, and the visual angle determined and compared with the angle of normal magnitude, *i. e.*, the angle subtending the fixed disc. The results are given under "Average Deviation from Normal Magnitude" in the tabulated statements. *Plus* indicates that the visual angle of the white disc was greater than that of the colored by so much, when they were judged to be equal; *minus*, that it was less; *i. e.*, in those marked *plus*, the color was overestimated, in *minus* it was underestimated.

In all the observations care was taken to exclude from the eye other light than that which entered through the observing-tube. The intensity of light was slightly greater on certain days than on others. But this made no appreciable difference in results, as was proved by a set of experiments undertaken for the purpose, with different light-intensities. Also the intensity-relation between the colored and uncolored disc was changed in order to see its influence on the judgment. The result was negative. There was made, *e. g.*, for the color blue, one series where the colored disc was compared with a white, composed of three tissue papers; while in an other series the white consisted of four tissue papers. The difference between the two series was smaller than the mean variation. This proves that irradiation and small differences in the relative light-intensities have under such circumstances, where bright objects are seen before an entirely dark background, no effect on the estimation of surface-magnitude. But after all, we chose our intensities as near to equality as it is possible to get them by comparison of differently colored surfaces in transmitted light. The two observed circles were exactly on a level with, each other to prevent the possibility

of one being enlarged by occupying a higher position, and being thus projected on a different retinal region. The difference in lateral indirect vision, too, was almost entirely excluded, by the circles being placed equally distant to the right and the left of the centre of vision. The trials were made throughout with the movable disc advancing and receding alternately, so that any possible error from this source was ruled out.

The tabulated results follow in Tables II-X (pages 33 to 38).

The conclusions previously reached were modified by an unexpected discovery, viz., that the moved circle was always underestimated. This is confirmed, too, by the trials in which both discs were white, the movable one, in order to appear equal to the other, requiring a visual angle greater by 28″.7 to 4′ 58″.8, and only in one instance less, and then but 5″.5. On an average the moved disc was underestimated by K. 1′ 31″.8, and by Q. 2′ 28″.8875. We found also, on thinking back, that the experimenter had in almost every case stopped, after judging the circles to be equal while slowly moving the one, and then observing them at rest was not satisfied with their equality, and again changed their relative positions a little. The underestimation of the surface-magnitude of a bright object on dark ground seems according to our experiments beyond all doubt. It is not the place here to enter on a further discussion of this phenomenon, the examination and explanation of which remain a subject for later investigation.

This consideration ought not to affect the total average with the colors where half the trials were made with the white disc moved (orange, blue-green, violet, purple); but with red, yellow, green and blue, the white was always at rest. Making the allowance which the trials with the two white circles indicate, *i. e.*, taking into account the average underestimation of the moved disc, and computing after this modification the ratios of the over or underestimation of the colored objects to the normal magnitudes, we arrive at the results in Table XI.

It may be of some interest also to compare the accuracy of the judgment for the different colors. For this purpose we

TABLE II.—*White.*

Division of Exps.	OBSERVER, DR. KIRSCHMANN.								OBSERVER, J. O. QUANTZ.							
	LEFT EYE.				RIGHT EYE.				LEFT EYE.				RIGHT EYE.			
	No. of Single Exp.	Av. Deviation from Norm. Mag		M. V.	No. of Single Exp.	Av. Deviation from Norm. Mag		M. V.	No. of Single Exp.	Av. Deviation from Norm. Mag		M. V.	No. of Single Exps.	Av. Deviation from Norm. Mag		M. V.
		° ′ ″		′ ″		° ′ ″		′ ″		° ′ ″		′ ″		° ′ ″		′ ″
Equal Discs	50	+0	2 8.7	1 14.92	50	+0	1 10.2	0 40.73	50	+0	0 46.55	0 34.9	50	+0	0 28.7	0 57.05
Unequal Discs	50	+0	2 53.8	1 41.8	50	−0	0 5.5	1 19.25	50	+0	4 58.8	1 16.65	50	+0	3 41.5	1 22.28
Av. for Single Eyes	100	+0	2 31.25	1 28.36	100	+0	0 32.35	0 59.99	100	+0	2 52.68	0 55.78	100	+0	2 5.1	1 9.66

	No. of Single Trials.	Average Deviation.		M. V.	No. of Single Trials.	Average Deviation.		M. V.
		Absolute Value.	Val. Rel. to Norm. Mag.			Absolute Value.	Val. Rel. to Norm. Mag.	
		′ ″		′ ″		′ ″		′ ″
Total Average	200	+1 31.8	+0.01355	1 14.18	200	+2 28.89	+0.0221	1 2.72

TABLE III.—*Red.*

Division of Exps.	OBSERVER, DR. KIRSCHMANN.								OBSERVER, J. O. QUANTZ.							
	LEFT EYE.				RIGHT EYE.				LEFT EYE.				RIGHT EYE.			
	No. of Single Exp.	Av. Deviation from Norm. Mag.		M. V.	No. of Single Exp.	Av. Deviation from Norm. Mag.		M. V.	No. of Single Exp.	Av. Deviation from Norm. Mag.		M. V.	No. of Single Exp.	Av. Deviation from Norm. Mag.		M. V.
		° ′ ″		′ ″		° ′ ″		′ ″		° ′ ″		′ ″		° ′ ″		′ ″
Red, Small,	25	—0	0 15.84	0 43.08	25	+0	1 26.11	1 6.45	55	—0	3 50.39	1 43.85	55	—0	2 28.74	1 22.20
Red, Large,	25	+0	0 2.44	1 29.	25	+0	3 33.24	1 8.2	50	—0	0 27.34	1 16.	50	+0	0 22.38	1 21.8
Av. for Single Eyes,	50	—0	0 6.70		50	+0	2 29.67		105	—0	2 8.86		105	—0	1 3.18	

TABLE IV.—*Yellow.*

Yellow, Small,	50	—0	2 27.71	1 35.02	50	+0	0 13.70	1 7.38	50	—0	4 1.23	0 55.4	50	—0	2 58.56	1 17.5
Yellow, Large,	50	—0	1 6.17	1 19.8	50	+0	1 7.27	1 15.73	50	—0	1 24.81	1 12.25	50	+0	0 5.16	1 19.85
Av. for Single Eyes,	100	—0	1 46.94		100	+0	40 48.77		100	—0	2 43.02		100	—0	1 26.70	

TABLE V.—*Green.*

Green, Small,	50	—0	3 57.26	1 52.27	50	—0	0 30.31	1 10.75	50	—0	2 51.86	1 33.8	50	—0	2 26.69	1 11.
Green, Large,	50	—0	4 43.76	1 43.15	50	—0	1 51.11	1 10.31	50	+0	0 7.91	0 57.93	50	—0	0 5.93	1 4.75
Av. for Single Eyes,	100	—0	4 20.51		100	—0	1 10.71		100	—0	1 21.97		100	—0	1 16.31	

TABLE VI.—*Blue.*

Blue, Small,	50	—0	4 35.03	1 23.05	50	—0	2 29.17	1 8.85	50	—0	3 13.23	1 19.29	50	—0	3 59.85	1 14.85
Blue, Large,	50	—0	5 45.07	1 34.93	50	—0	1 48.41	2 12.05	50	—0	1 58.14	1 13.	50	—0	0 55.08	1 41.3
Av. for Single Eyes,	100	—0	5 10.05		100	—0	2 8.79		100	—0	2 35.68		100	—0	2 27.46	

TABLE III.—*Red.*

	No. of Single Trials.	Average Deviation.		M. V.	No. of Single Trials.	Average Deviation.		M. V.
		Absolute Value.	Val. Rel to Norm. Mag.			Absolute Value.	Val. Rel. to Norm. Mag.	
		′ ″		′ ″		′ ″		′ ″
Total Average,	100	+1 11.49	+0.0117	1 6.68	210	—1 36.02	—0.0155	1 25.96

TABLE IV.—*Yellow.*

Total Average,	200	—0 33.23	—0.0053	1 19.48	200	—2 4.86	—0.0201	1 11.25

TABLE V.—*Green.*

Total Average,	200	—2 45.61	—0.0269	1 29.12	200	—1 19.14	—0.0127	1 10.87

TABLE VI.—*Blue.*

Total Average,	200	—3 39.42	—0.0356	1 34.72	200	—2 31.57	—0.0245	1 22.11

TABLE VII.—*Orange.*

Division of Expers.	OBSERVER, DR. KIRSCHMANN.									OBSERVER, J. O. QUANTZ.								
	LEFT EYE.				RIGHT EYE.					LEFT EYE.				RIGHT EYE.				
	No. of Single Exp.	Av. Deviation from Norm. Mag.		M. V.	No. of Single Exp.	Av. Deviation from Norm. Mag.		M. V.		No. of Single Exp.	Av. Deviation from Norm. Mag.		M. V.	No. of Single Exp.	Av. Deviation from Norm. Mag.		M. V.	
		° ′ ″		′ ″		° ′ ″		′ ″			° ′ ″		′ ″		° ′ ″		′ ″	
I. Right, Large,	25	+0	9 3.95	1 38.25	25	+0	6 26.94	1 44.18		25	+0	3 43.95	1 27.05	25	+0	2 54.1	1 13.15	
II. Right, Small,	25	+0	6 41.8	1 13.28	25	+0	4 1.95	1 18.44		25	+0	1 37.3	1 19.75	25	+0	1 25.6	0 59.05	
III. Left, Large,	25	—0	0 16.26	0 55.8	25	+0	2 11.95	1 7.58		25	—0	0 48.5	0 59.78	25	+0	0 40.9	1 0.85	
IV. Left, Small,	25	—0	0 37.3	1 23.63	25	+0	3 16.9	1 10.25		25	—0	4 32.95	1 16.68	25	—0	2 29.25	1 16.35	
Av. for Single Eyes,	100	+0	3 43.05	1 17.74	100	+0	3 59.44	1 20.11		100	—0	0 0.05	1 15.81	100	+0	0 37.84	1 6.85	

TABLE VIII.—*Blue-Green.*

I	25	+0	4 24.7	1 17.23	25	—0	0 1.8	1 24.7		25	+0	3 13.85	1 37.3	25	+0	1 2.2	1 23.78	
II	30	+0	4 9.8	1 6.95	30	+0	0 36.8	1 37.78		30	+0	0 50.4	1 1.4	30	—0	0 35.55	1 3.88	
III	25	—0	6 40.4	1 20.4	25	—0	4 34.4	1 22.2		25	—0	0 22.5	1 43.58	25	—0	0 28.9	1 23.05	
IV	25	—0	6 10.8	1 23.8	25	—0	3 28.35	2 10.53		25	—0	2 59.1	2 1.	25	—0	2 13.65	1 33.08	
Av. for Single Eyes,	105	—0	1 4.18	1 17.10	105	—0	1 51.94	1 38.80		105	+0	0 10.66	1 35.82	105	—0	0 33.98	1 20.95	

TABLE IX.—*Violet.*

I	25	+0	3 12.1	1 46.5	25	+0	1 11.25	1 34.75		25	+0	3 2.	0 55.70	25	—0	0 1.8	1 33.63	
II	25	+0	3 34.4	1 8.38	25	+0	1 56.3	0 54.36		25	+0	0 13.1	0 51.55	25	—0	0 39.15	1 20.93	
III	25	—0	7 5.45	1 1.75	25	—0	5 4.35	1 24.7		25	—0	1 2.77	1 4.25	25	—0	0 13.55	1 1.7	
IV	25	—0	5 39.5	1 16.33	25	—0	3 27.1	1 55.93		25	—0	4 35.6	1 39.93	25	—0	3 34.5	1 15.	
Av. for Single Eyes,	100	—0	1 29.61	1 18.24	100	—0	1 20.98	1 27.43		100	—0	0 35.82	1 7.86	100	—0	1 7.25	1 17.81	

TABLE VII.—*Orange.*

	No. of Single Trials.	Average Deviation.		M. V.	No. of Single Trials.	Average Deviation.		M. V.
		Absolute Value.	Val. Rel. to Norm. Mag			Absolute Value.	Val. Rel. to Norm. Mag.	
		° ′ ″		′ ″		° ′ ″		′ ″
Total Average,	200	+0 3 51.24	+0.0358	1 18.92	200	+0 0 37.79	+0.0031	1 11.33

TABLE VIII.—*Blue-Green.*

Total Average,	210	—0 1 28.06	—0.0138	1 27.95	210	—0 0 11.66	—0.0018	1 28.38

TABLE IX.—*Violet.*

Total Average,	200	—0 1 25.29	—0.0134	1 22.84	200	—0 0 51.53	—0.00785	1 14.75

TABLE X.—*Purple.*

Division of Exps.	OBSERVER, DR. KIRSCHMANN.								OBSERVER, J. O. QUANTZ.							
	LEFT EYE.				RIGHT EYE.				LEFT EYE.				RIGHT EYE.			
	No. of Single Exp.	Av. Deviation from Norm. Mag.		M. V.	No. of Single Exp.	Av. Deviation from Norm. Mag.		M. V.	No. of Single Exp.	Av. Deviation from Norm. Mag.		M. V.	No. of Single Exp.	Av. Deviation from Norm. Mag.		M. V.
		° ′ ″		′ ″		° ′ ″		′ ″		° ′ ″		′ ″		° ′ ″		′ ″
I	50	+0	8 2.1	1 39.9	50	+0	5 45.	1 33.	50	+0	4 41.	1 15.	50	+0	3 10.1	1 24.35
II	50	+0	3 12.9	0 53.9	50	+0	1 27.6	0 35.9	50	+0	1 34.8	1 13.5	50	+0	0 19.6	1 7.45
III	50	−0	5 35.6	1 4.55	50	−0	3 19.9	1 17.5	50	−0	0 45.5	1 43.3	50	+0	0 32.6	1 44.6
IV	100	−0	3 39.98	1 43.55	100	−0	1 9.85	1 38.91	100	−0	3 17.65	1 26.33	100	+0	0 34.85	1 20.41
Av. for Single Eyes,	250	+0	0 29.85	1 20.48	250	+0	0 40.71	1 16.33	250	+0	0 33.16	1 24.53	250	+0	1 9.29	1 24.20

	No. of Single Trials.	Average Deviation.		M. V.	No. of Single Trials.	Average Deviation.		M. V.
		Absolute Value.	Val. Rel. to Norm. Mag.			Absolute Value.	Val. Rel. to Norm. Mag.	
		° ′ ″		′ ″		° ′ ″		′ ″
Total Average,	500	+0 0 35.28	+0.0045	1 18.40	500	+0 0 51.23	+0.0078	1 24.37

TABLE XI.

COLOR.	OVER- OR UNDER-ESTIMATION. RATIO TO THE NORMAL MAGNITUDE.	
	Observer K.	Observer Q.
Red,	+0.02525	+0.0066
Orange,	+0.0358	+0.0031
Yellow,	+0.00825	+0.0020
Green,	—0.01335	+0.0094
Blue-green,	—0.0138	—0.0018
Blue,	—0.02205	—0.0024
Violet,	—0.0134	—0.00785
Purple,	+0.0045	+0.0078

TABLE XII.

COLOR.	OBSERVER K.		OBSERVER Q.	
	AVERAGE MEAN VARIATION		AVERAGE MEAN VARIATION.	
	Absolute Value.	Ratio of the Normal Magnitude	Absolute Value.	Ratio of the Normal Magnitude
Red,	1′ 6″.681		1′ 25″.9625	
Orange,	1′ 18″.92375		1′ 11″.3306	
Yellow,	1′ 19″.481		1′ 11″.25	
Green,	1′ 29″.1225		1′ 10″.87	
Blue-green,	1′ 27″.9481		1′ 28″.38375	
Blue,	1′ 34″.719		1′ 22″.111	
Violet,	1′ 22″.835		1′ 14″.7469	
Purple,	1′ 18″.4016		1′ 24″.3672	
Av. of all Colors,	1′ 22″.264	0.0131 (= ca. $\frac{1}{76}$)	1′ 18″.6278	0.0125 (= ca. $\frac{1}{80}$)
White,	1′ 14″.175	0.0110	1′ 2″.71875	0.0093 (= ca. $\frac{1}{108}$)

have in Table XII placed together the average mean variations, which remain without exception between the limits of 1′ and 1½′. The accuracy in case of the comparison of two white discs is only very little greater than that of the comparison of a colored disc with a white one. Among the 104 series of experiments there are only three cases where the m. V. is greater than 2′, and fifteen cases where it amounts to less than 1′. The greatest value for the m. V. occurred in the experiments with blue (Observer K.), and amounts to 2′ 12″.05, which corresponds to a relative value of 0.0216 of the normal magnitude. The smallest m. V. we find in the experiments with purple = 35″. 9, that is, 0.0059 of the normal magnitude, or a little more than the smallest visual angle which can be perceived in colored light.[1] It may be remarked here, that everywhere in our tables, where we give the average in minutes and seconds, these absolute values contain a certain small inaccuracy, because the averaged values refer to different normal magnitudes. (For the colors red, yellow, green and blue, the normal magnitudes are 6227″.3 and 6106″; for the other colors they are 6741″.8 and 6099″.3; in the case of two white discs the normal magnitude was always 6741″.8.) In the relative values reported in our tables, this inaccuracy is eliminated.

If we review the results of our investigation in the condensed form given in Tables XI and XII, we are able to draw the following conclusions concerning the influence of the quality of light-sensation on the estimation of surface magnitudes:

There is a small, but decided, influence of color. Red, orange, yellow and also purple have been overestimated by both observers, while blue-green, blue and violet show a decided underestimation. The color of the middle of the spectrum only, green, has different effects on the two observers. The over and underestimation respectively, although they may take part in causing optical illusions, are not con-

[1] Uhthoff, "Ueber die kleinsten wahrnehmbaren Gesichtswinkel in den verschiedenen Theilen des Spektrums." *Zeitschrift für Psychologie und Physiologie der Sinnesorgane*, B. I, p. 155 ff.

siderable enough to explain a phenomenon like that of the rising moon. They are indeed very small, varying between the limits of $\frac{1}{28}$ and $\frac{1}{220}$ of the normal magnitude for the Observer K., and $\frac{1}{106}$ to $\frac{1}{560}$ for the Observer Q. But they present themselves with a marked regularity and constancy, and with a decided coincidence of their direction in the results of the two observers.

In order to secure the commensurability of the results for the different colors, it was necessary to apply for all colors approximately the same normal magnitude, which was less than 2° (= about 4 diameters of the full moon) and more than $1\frac{1}{2}$° (or about 3 diameters of the moon). It is possible that the over or underestimation would be found to be more considerable in case of smaller normal magnitudes.

Résumé.

I. When colored surfaces of moderate size are seen on a darker background, the colors of the less refrangible part of the spectrum, and also reddish purple, show a decided tendency towards overestimation in space-extension, while for the more refrangible colors of the spectrum a marked underestimation takes place.

II. Our judgment of the equality of surface-magnitudes shows a rather high degree of accuracy, which is for white but little greater than for colored surfaces.

III. White or colored surfaces of moderate size, seen on a dark background, are underestimated in size when seen in motion towards or from the eye.

MINOR STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF CORNELL UNIVERSITY.

COMMUNICATED BY E. B. TITCHENER.

XI.—SOME QUESTIONS OF THE CUTANEOUS SENSIBILITY.

BY W. B. PILLSBURY, A. B.

INTRODUCTION.

This article gives the results of a series of experiments made during the academic year 1893-94, at the Cornell Psychological Laboratory. The method employed was the second of those used by E. H. Weber in his classic investigation of cutaneous space relations. A point on the skin was touched and the subject requested to indicate the point stimulated as accurately as possible. The average error of such attempts at localization afforded a relative measure of the space sensibility of the skin. Weber himself considered this average error an absolute measure of the local sensibility of the part of the skin worked upon. He says: "Bestimmt man mit einem Zirkel und Maasstabe wie weit der Beobachter von dem gesuchten Orte entfernt bleibt, wenn er demselben am nächsten zu sein glaubt, und nimmt aus vielen solchen Bestimmungen das Mittel, so wird man finden, dass es desto weiter von ihm entfernt bleibt je unvollkommener der Raumsinn in dem Theile der Haut ist an welchem der Versuch gemacht wird.[1]"

1. Czermak objected that this average error must be in every case too small, and therefore rejects the method entirely. He says:[2] "Bei dieser Bestimmung spielt der Zufall eine so bedeutende Rolle dass der Werth des ganzen Verfahrens zur Ermittlung der Feinheitsgrade des Raumsinnes in Frage gestellt wird. * * * Hier hilft es auch

[1]Ueber den Raumsinn, pp. 89-90. In the *Verhandlungen d. k. sächsischen Gesellschaft d. Wissens. in Leipzig. Math.-phys. Classe*, 1852.

[2]Physiologische Studien, 2te Folge, pp. 52-53.

nicht das Mittel aus vielen Beobachtungen zu ziehen, da die Bestimmungen in überwiegender Zahl zu klein sind, das Mittel daher auch zu klein ausfallen muss. Bei dem Verfahren mit dem Zirkel ist die Bestimmung durch zwei Grenzwerthe, einen kleinsten und einen grössten beschränkt, während bei diesem Verfahren nur der eine und zwar der grösste Grenzwerth nicht überschritten werden kann, indem es für dieses Verfahren gar keinen kleinsten Grenzwerth gibt. Der kleinste Grenzwerth ist hier = 0, wenn nämlich die suchende Sonde zufällig die zuerst berührte Hautstelle findet. * * * Aus dem gesagten ergibt sich nun von selbst dass das zweite Weber'sche Verfahren zur genaueren Bestimmung der Feinheit des Raumsinnes in der Haut gänzlich unbrauchbar sei."

In his statement of the facts, Czermak is unquestionably correct. The average error will necessarily be smaller than the limen of twoness. But his conclusion that the method is for this reason useless is not so unexceptionable. For, although itself too small, the error must nevertheless bear a constant and mathematically determinable relation to the limen. It is plain that within the figure formed by the limen, one point would be hit upon as often as any other in an infinite number of experiments. Within the limen there is no diverting agency which would tend to favor one point rather than any other. All would be determined by the laws of chance. The case is not analogous to that of shots fired at a target, where there is a conscious endeavor to hit the centre. For the consciousness of the subject, the figure bounded by the limen is a mathematical point. Centre and circumference are one for him. Consequently, his attempt to touch the figure bounded by the limen has no effect in directing a point towards the centre rather than towards any other point within its area.—It might be supposed, on the other hand, that the periphery would be favored; approach being usually made from the outside. But the localization is frequently made from above, and at the first attempt. Even when a point outside the limen is first touched, and the correction made by moving along the surface, the exploring pencil generally moves around in all directions in the neighborhood of the point sought, and often finally stops only on the far side of it. This lack of directing agency is not only theoretically demonstrable, but is proved by an examination of our results.[1]

If we should average the errors made on every radius of the

[1] The absence of preference of the periphery in this context of *amount* of localization error does not, of course, conflict with the tendency to localize in a certain *direction*, noticed below.

figure formed by the limen, and plot on the radii the averages thus obtained, we should have a line bounding a figure similar to that bounded by the limen and dividing the area of the limen into equal parts. This line of average error would, *i. e.*, contain the same area within it as was contained between it and the limen. By the law of similar figures, the distance from the point touched to any point on the line of average error would be to the distance from the same point to the corresponding point on the limen as 1 to $\sqrt{2}$. From this it is evident that the mean error of localization can be used as a measure of discrimination for the comparison of different points of the skin; and its reduction (multiplication by $\sqrt{2}$) will give a value comparable with the value of the limen as determined by other methods. This, then, is one of the points to which we desire to call attention in the present investigation.

2. Another of the principal objects of the investigation was the determination of the part played in localization by the visual image, which the reagent *Wn.*[1] had already found to be an important factor in all cutaneous space judgments. To obtain this a number of series were made with the subject localizing so far as possible in terms of tactual sensations alone, followed by a number of series in which as much prominence as possible was given to the visual image. In order still further to increase the prominence of the visual image, the person experimented upon, during series of a third type, kept the eyes open and fixed on the arm during the application of the pressure, and then closing the eyes localized as before.

3. A third problem was the testing of the results obtained with the photograph-method of Henri. In this the subject localized the point touched upon a photograph of the arm instead of upon the arm itself. Here again we find the visual image playing an important rôle, but under several new and complicating influences.

Experiments.

Our investigation was restricted to an area of the volar side of each forearm, extending from the folds of the skin at the base of the palm to a point some nine centimeters up the arm towards the elbow. This area was subdivided into twelve by two longitudinal and three horizontal lines. Only one experiment was made on each of these parts during a single

[1] *Ueber den Einfluss der Gesichtsassociationen auf die Raumwahrnehmungen der Haut. Phil. Stud. XI, 2.* As Miss Washburn's paper is logically prior to the present, the latter has been withheld from publication until the appearance of the *Studien*.

experimental series, as it was found that the after-effect of pressure exerted a disturbing influence upon judgment. It was often noticed during the first few days of experimentation, when this point had not been observed, that the after-effect of a preceding experiment was mistaken for the impression given to be localized. Even when this mistake was not made, the subject was conscious of a confusion due to the same cause.

During experimentation the reagent sat with eyes closed or open, as the method required, the arm resting comfortably on a table. The experimenter touched a point on the skin with a charcoal point; the reagent indicated the point touched with another and similar charcoal point. The error made in this localization was then measured with a compass and scale. Care was taken that the charcoal points should be of the same size—one millimeter in diameter. Record was made both of amount and direction of error. The directions were divided into eight groups, for convenience of record: right (R.), left (L.), peripheral or towards wrist (P.), central or towards elbow (C.), and the directions midway between these: *L. P.*, *R. P.*, *L. C.*, and *R. C.* The subjects were Miss Washburn (*Wn.*), Messrs. Knox (*K.*), Read (*R.*), Titchener (*T.*) and Watanabe (*We.*). With the exception of *R.*, all had had experience in experimental work, and all the rest except *K.* in experiments on this same portion of the arm.

All methods of collecting the results from a limited number of experiments are liable to objection. Two methods of massing the experiments from different positions are open to us. The results may be averaged, each result counting for one, without reference to the position of stimulation; or the average may be obtained for each position and then the average values of the various positions averaged. The first method may be very unfair to some particular position, since the errors are determined in their position by chance, and the sensibility of the skin is very different for different positions. There might, for instance, be a large proportion of all the errors towards the wrist (*P.*) made at some point of great sensibility. If averaged directly, the value for *P.* in the given case would be much too small. This objection would, of course, hold against any method of determination in which care was not taken that an equal number of experiments were made on each portion of the part of the skin which was being investigated. The other method allows the fewer experiments equal weight with the larger number, and in a very limited number of experiments may give rise to serious errors. These, however, are at most chance errors, and will

disappear with an increase in the number of observations. As the lesser of the two evils, we have chosen the latter method of 'massing.'

1. The results obtained from two reagents in one investigation of the *effect of visualization*, massed as explained above, are given in Table I.

TABLE I. *Unit = 1 mm.*

Reagent T.			*Reagent Wn.*		
Without Viz.	With Vis.	EyesOpen.	With'tVis.	With Vis.	EyesOpen.
7 Series. 168 Ex.	7 Ser. 168 Ex.	8 Ser. 192 Ex.	19 Ser. 456 Ex.	19 Ser. 456 Ex.	20 Ser. 480 Ex.
P. = 8.35	4.09	3.95	4.26	4.56	3.03
C. = 8.59	4.76	5.20	4.76	5.03	2.84
R. = 6.65	2.45	3.18	2.28	3.52	2.35
L. = 7.57	5.11	3.27	3.38	3.49	2.62
R. P. = 6.78	4.80	4.49	4.10	5.19	3.00
L. P. = 6.88	4.92	5.28	3.03	3.58	3.79
R. C. = 9.03	5.28	4.78	5.03	4.62	2.78
L. C. = 7.13	6.88	5.14	5.21	2.95	2.74
60.98	38.29	34.39	32.37	32.94	23.15

We give only the results from *T.* and *Wn.* All the others show the same tendencies as *Wn.* The sums of all the errors are given merely for convenience, this appearing the best means of showing at a glance the relations between the different series. Nothing is claimed for them as averages.

In the case of every reagent the experiments of the first seven or eight days were disregarded, so that practice may be considered constant throughout the entire table. The series of each day was not long enough to give rise to fatigue. The experiments seldom required more than half an hour a day.

The results show that none of the reagents, with the exception of *T.*, were able voluntarily to control *visualization.* All, during the experiments themselves, frequently said that they found it impossible to shut out the visual image, and

their results show no difference between the experiments performed with and without visualization. *T.*, however, found it possible to exclude the visual image to a large extent, was positive of the fact as indicated by introspection during the observations, and shows in his results a very marked difference between the two methods.

2. As regards the influence of *position*: we find the longitudinal divisions to be about on a par, so far as can be decided from the experiments performed. Right, left and centre of the wrist seem to show an equal error. In several cases there was an indication of a slight superiority on the outside of the arm, *i. e.*, the right side of the right arm and the left side of the left; but the results are not concordant enough to be decisive on this point.

The differences between the horizontal divisions are more striking, as will be seen by a glance at Table II. In this table the Roman figures designate the distance from the base of the palm at which the observations were made; I. beginning at a distance of approximately 9cm., and the others approaching the palm by stages of about 3cm. Absolute exactness of stimulus position is not claimed, but the variation did not exceed $\pm$ 2.5 mm.

The first quantity at the foot of the column gives the sum of the errors in the position. It affords a good indication of the gradual decrease of the error as the wrist is approached. The second figure shows the relation between the horizontal and the vertical errors. In this fraction the numerator represents the horizontal, the denominator the vertical errors.

There are several exceptions to the general rule of the increase of the ratio between horizontal and vertical errors as the wrist is approached, but such exceptions are not more numerous than would be expected with such a small number of results.

Only selected results are given, to avoid too great length. The series chosen were from the more experienced subjects, and are those which include the greatest number of experiments. Where other means of preference were lacking, results were chosen that fairly represented the whole number. There is only one exception to the general law of the relation between horizontal and vertical errors, and none to the tendency to decrease toward the wrist, in the series of results not given.[1]

In this table we see a constant decrease in the size of the error as we go toward the wrist. The greatest difference is either between the series taken near the base of the hand, on

[1] No. of series published = 6. No. of series not published = 3.

TABLE II. *Unit = 1 mm.*

Reagent T.

	WITH VIS. 7 Series. 168 Experiments.				EYES OPEN. 8 Series. 192 Experiments.			
	I.	II.	III.	IV.	I.	II.	III.	IV.
P. = 5.83	4.54	4.84	2.00	6.28	5.19	3.06	2.49
C. = 5.84	6.85	3.25	3.72	7.38	6.09	3.84	3.00
R. = 1.00	3.45	2.62	3.39	2.97	3.67	3.02	2.79
L. = 3.13	3.75	4.50	3.75	Lacking	1.95	3.00	3.30
R.P. = 4.00	4.00	6.50	5.15	5.32	3.75	Lacking	3.00
R.C. = 6.08	4.15	5.13	Lacking	7.00	3.75	4.00	5.25
L.P. = 4.50	5.63	5.00	"	8.63	5.17	3.22	5.13
L.C. = 10 25	6.17	7.38	3.50	3.00	5.63	4.10	4.00
	41.03	38.54	39.22	29.02[1]	46.37[1]	35.20	27.70[1]	28.96
	Ratio: 0.3530	0.6321	0.8801	1.2518	0.4349	0.4982	0.8725	1.1109

Reagent R. *Reagent We.*

	EYES OPEN. 7 Series. 168 Experiments.				NORMAL. 13 Series. 312 Experiments.			
	I.	II.	III.	IV.	I.	II.	III.	IV.
P. = 5.20	5.56	4.33	1.70	9.67	8.13	6.26	5.75
C. = 5.36	4.54	4.50	2.25	4.25	4.25	4.63	1.88
R. = 7.25	2.00	2.33	1.50	5.42	5.32	3.03	2.69
L. = 4.25	4.13	5.00	1.23	6.00	5.14	5.09	4.84
R.P. = 4.50	3.25	5.67	5.00	8.63	7.13	7.50	6.75
R.C. = 8.00	4.37	1.00	4.75	7.00	5.00	4.50	4.10
L.P. = 5.84	7.25	3.50	3.50	8.75	6.17	6.70	3.54
L.C. = 5.50	Lacking	4.50	3.45	7.15	7.09	4.00	4.80
	45.90	35.55[1]	30.83	25.38	56.81	48.23	41.71	34.40
	Ratio: 1.0890	0.6069	0.8301	0.6963	0.8204	0.8449	0.7457	09.934

[1]The directions lacking are supplied by the average value.

Reagent Wn.

Without Vis. (Theoretically.) 19 Series. 456 Experiments.				Eyes Open. 20 Series. 480 Experiments.			
I.	II.	III.	IV.	I.	II.	III.	IV.
P.=5.17	3.89	3.89	3.47	4.32	3.68	2.95	1.72
C.=5.10	4.15	4.68	3.27	3.50	3.84	3.18	1.84
R.=2.50	1.12	4.64	2.83	3.89	1.38	2.00	1.96
L.=3.92	3.39	4.59	2.17	2.44	2.14	3.09	2.36
R.P.=3.09	5.03	3.38	3.34	4.00	2.75	1.24	2.07
R.C.=5.26	5.27	5.50	4.00	2.50	1.63	3.00	4.50
L.P.=3.43	4.60	4.04	2.94	4.50	4.34	2.23	2.03
L.C.=6.83	6.07	4.76	3.17	2.25	3.55	2.38	2.00
35.30	33.52	35.48	25.19	27.40	23.31	20.37	18.68
Ratio: 0.5278	0.5609	1.0770	0.7418	0.8095	0.4681	0.8303	1.2135

the folds of the skin at the joint, and the other three; or between the two upper and the two lower sets of observations. This difference is due, at least in part, to individual variations in the distance from the hand to which the folds extend. Not only does the size of the error decrease as the folds of the wrist are approached, but the form of the figure constituted by the limen changes in a marked degree. On the parts of the arm nearer the elbow, the longer axis of the ellipse formed by the limen is vertical; on the folds of the skin at the joint, the horizontal axis bears a much larger proportion to the vertical axis, and in some cases becomes the long axis of the figure.

This change of direction seems in some way connected with the direction of the prominent markings, cords or folds, on the surface of the skin. The greatest error is made in the direction of such markings. That is, the greatest error is longitudinal on the higher parts of the arm, while near the base of the palm, where the folds also enter as prominent visual landmarks, we find the horizontal errors increase and in some cases become predominant in the final result. Moreover, during an experiment the reagent was often con-

scious of localizing by means of the image of the cords or folds. In several cases where an unusually large error was made, he would remark that it was 'on the same cord,' without knowing that the error made was exceptional. In a few instances it was found that one cord was mistaken for another.

The most obvious explanation is to be given in terms of the effect of visualization. The 'local signs' of the skin seem to be translated by association into terms of the visual image, and the localization made by means of a second association with the local signs. The experiment seems to be a search for a sensation of the same local sign as the original sensation. In this search the observer is first, and, in a general way, assisted by the association formed with the visual image, and through this with the appropriate motor sensation. As the exploring point touches the skin the local signs call up the associated visual image in terms of which, principally, the direction of the error is noted and the necessary corrections made. When a local sign and its associated visual image coincide with the local sign and visual image originally given, and for which the observer is seeking, the localization is considered as complete. In most cases, however, the local sign is to a great extent lost sight of, and the comparison takes place almost wholly in terms of the visual image alone. In such cases one would expect the result found in the experiments, that similarity in visual form should be accepted as identity in position. The great aid rendered by the visual image was noticed and frequently remarked upon by the reagents. They declared that they saw the point touched upon a mental visual image and used this image as a chart in their localization.

Another possible hypothesis might ascribe to the form of the surface a power of affecting the character of local signs. This would necessarily be in terms of the effect of physical structure on the transmission of the mechanical stimulus from the point touched to the nearest end-organs of touch. But the effect on the nerve endings could only be to give a difference in intensity, not in quality, and it seems impossible to frame a schema in accordance with which such delicate distinctions could be made by an organ of such comparatively gross sensibility.

3. In Table III. we give the average error, corrected as explained at the beginning of this article, for the error due to chance, *i. e., the value of the localization limen*. The values for the upper part of the arm and for the folds of the skin are given separately. During the first experiments, no record was kept of the part of the wrist on which the error was made. For these series the average for the whole area of

skin investigated is given (column 3). In making the correction, only 1.4 of the value of $\sqrt{2}$ is used, as giving a sufficiently accurate result.

Throughout the Table, the Roman figures, as before, show the distances above the wrist at which the series were taken. I. is nearest the elbow, (about 9 cm. above the base of the palm); the others are successively 3 cm. farther from it.

The relatively small value of the limen, as determined by this method, must be in part due to the fact that the exploring point is moving over the skin, in part to the better attention of the reagent assured by the movements he must make, and in part to the additional aid rendered by associated movement sensations.

The increase of the error during visualization in *We.'s* results was probably due to the disturbing effect of introspection, and the attempt at control. It is to be noticed that in his case it is a comparison of normal localization, without regard to the visual image, and attempted visualization. With the other reagents there is an effort to visualize or not to visualize in the two series.

TABLE III.

Unit = 1 mm.		Mas'd I-III.	IV.	Mas'd I-IV.
T.	Without Visualization.	——	——	10.67
	With Visualization.	6.93	5.08	——
	Eyes Open.	6.25	5.06	——
R.	With Visualization.	8.81	6.93	8.91
	Eyes Open.	6.55	4.08	——
We.	Normal.	8.57	6.02	——
	With Visualization.	9.27	6.47	——
K.	Normal.	6.86	3.47	7.55
Wn.	Without Visualization.	6.09	4.41	——
	Eyes Open.	4.14	3.63	
	With Visualization.	——	——	5.94

4. Another interesting feature of the investigation was the constancy of the tendencies controlling the *direction of the error*. There are, evidently, several factors at work in the determination, within the limen, of the direction which the error will take. Some of these the investigation afforded a means of analyzing out. The most noticeable tendency was a displacement towards the wrist. The number of errors in

this direction is greater on the outside of the arm than on the inside, greater on the left arm, where the localization was made with the right hand, than on the right arm. The results from two reagents, tabulated to show this tendency, are given in Table IV. Here again we give the results from the reagents from whom we have the greatest number of observations. The results from the three methods, with visualization, without visualization, and with eyes open, are massed for convenience. All separately show the same tendencies. In collecting the results, the errors in the oblique directions were halved and one-half added to each of the principal directions between which the error was situated. In the abbreviations at the top of the columns, the first letter designates the hand, left or right, on which the experiment was made; the second indicates the part (left, centre, right) of the arm stimulated. The per cents. at the bottom of the columns show the ratio of peripheral errors to all errors made in the vertical line.

TABLE IV. Unit = 1 mm.

Reagent Wn. 58 series; 1,392 experiments.

L. L.	L. C.	L. R.	R. L.	R. C.	R. R.
P. = 127½	*P.* = 93½	*P.* = 67½	*P.* = 35	*P.* = 65½	*P.* = 87½
C. = 36	*C.* = 38½	*C.* = 65	*C.* = 87	*C.* = 63	*C.* = 66
78%	71%	51%	28%	51%	57%

Reagent T. 20 series; 480 experiments.

L. L.	L. C.	L. R.	R. L.	R. C.	R. R.
P. = 39½	*P.* = 48	*P.* = 26½	*P.* = 23½	*P.* = 22½	*P.* = 21½
C. = 22	*C.* = 14½	*C.* = 21	*C.* = 28	*C.* = 20	*C.* = 29
64%	76%	56%	46%	53%	41%

One important factor in causing this displacement towards the wrist is probably the overestimation of movements due to flexion and underestimation of those due to extension, when the arm is much flexed. As the reagent ordinarily sits at

the table with one arm resting on it, the other arm is naturally flexed nearly to its limit in making the localization. This view is strongly supported by the experiments of Loeb.[1] In exhaustive experiments made on the estimation of distance by movements of the hand, Loeb finds that there is always an overestimation where the muscles that give the initial movement are already contracted, and an underestimation where they are extended; *i. e.*, movements in flexion, where the limb is much flexed, are overestimated, and underestimated where the limb is much extended; for movements in extension the reverse is true. In translation from visual space (in terms of eye movements) into motor space (with hand movement), there is the same underestimation of movements of extension, increased by overestimation of the space moved through by the eye. In both cases the problem seems to be the same as our own—to determine the errors which arise when translating from the visual space diagram into muscular movements. In the second case, the results were affected by the divergence of the physiological visual space in the remoter parts of the field of vision from the normal visual space, while in our experiments, the normal visual space (which also probably formed the measure in Loeb's first case) was called up directly by the tactual impressions. We must replace Loeb's explanation of the phenomenon, that the errors in estimation are due to the differences in the extent of movements for the same amount of innervation, by an explanation in terms of a subjective equality between distances in visual space and the amount of sensations from the moving member. It is not that equal innervation sensations correspond to equal lengths of movement, but that equal lengths of movement give greater amounts of motor sensations, and these are taken to mean greater distances in the visual space diagram.

Of remotely related literature there is a little. Münsterberg's[2] pleasure-pain experiments on flexion and extension were evidently performed with the arm more extended, and, hence, would have no weight as compared with this position of the arm. Krohn's[3] method, if fully made use of, would give important evidence as to whether the error is due to the cutaneous sensibility or "muscle sense," but the results are recorded in such a schematic manner and the whole investigation is so uncritical and apparently so inaccurate that but

[1] *Untersuchungen über den Fühlraum der Hand.* Pflüger's Arch., XLI, pp. 107-127. *Untersuch. über d. Orientirung im Fühlraum d. Hand u. im Blickraum.* Ibid., XLVI, pp. 1-46.

[2] Beiträge, 4tes Heft, pp. 216 ff.

[3] *Journal of Nervous and Mental Diseases*, March, 1893.

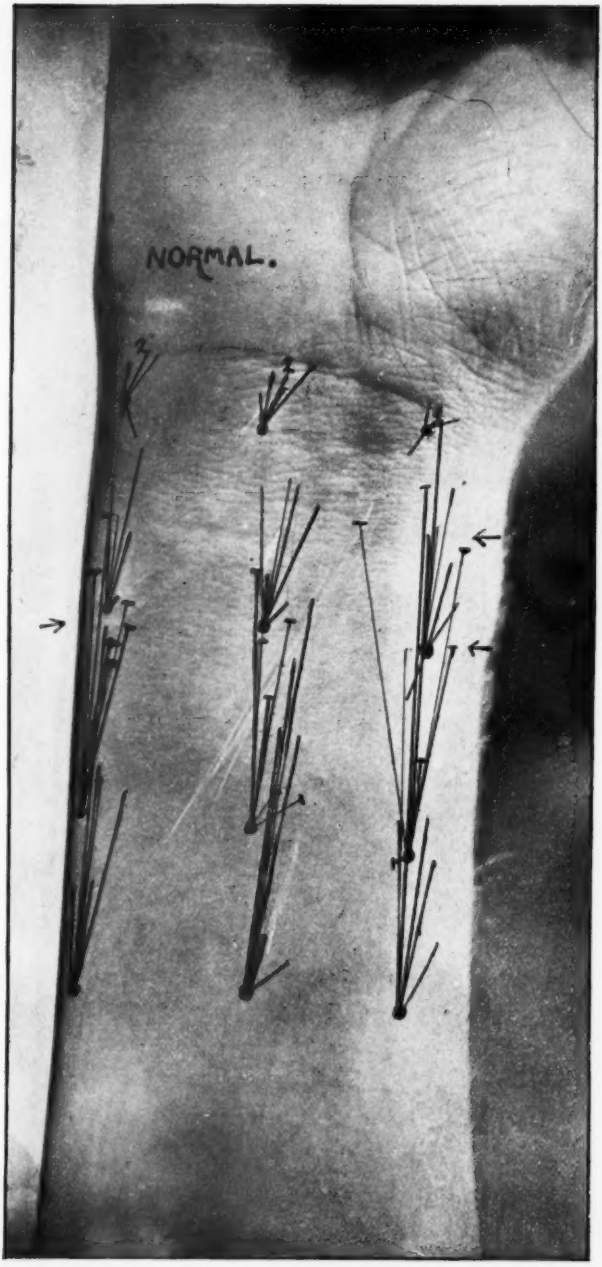
little can be obtained from it. Cattell and Fullerton[1] established the fact that small distances were overestimated and large underestimated. This probably is one of the more general laws under which these facts of displacement come. At extremes of flexion, further flexion gives more intense sensations (increases strain sensations in sinew and probably sensations of contraction in muscle also) than the same extent of movement in extension. Quantity of sensation is probably in part correlated in consciousness with extent of movement. We should then have a positive error in localization during extension, a negative error during flexion, and the point would be localized as we find it in our experiments, nearer the wrist than the point sought.

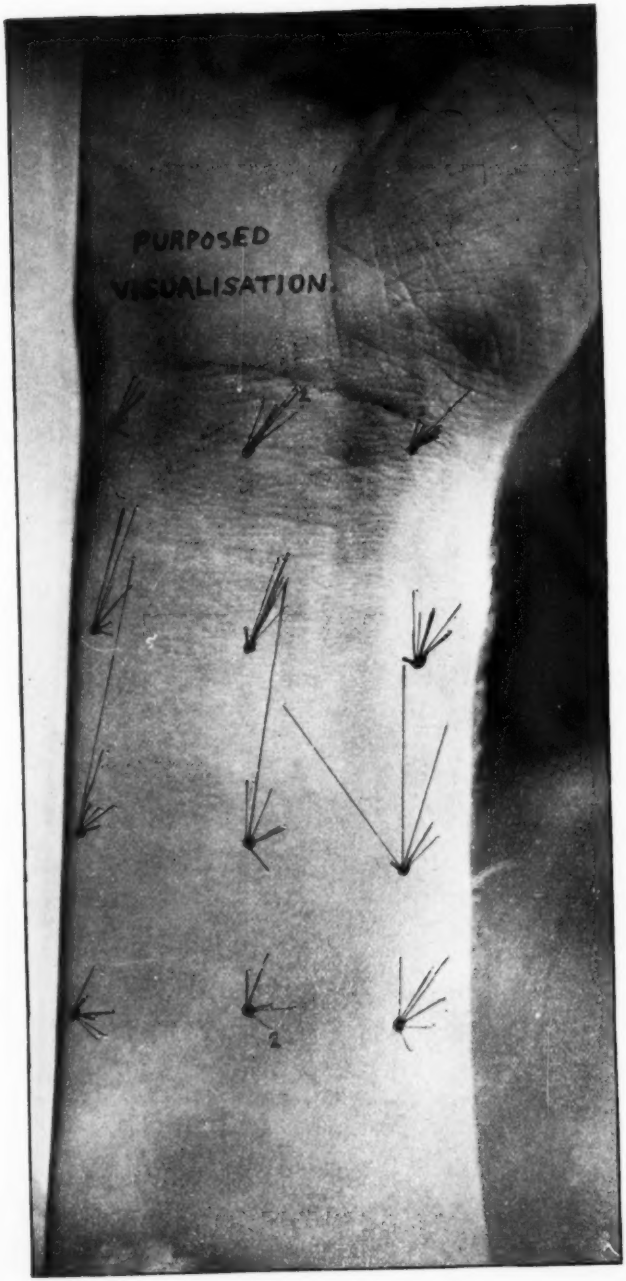
Further evidence for this hypothesis is given in our results by the increase of displacement as one proceeds from the inside of the arm (right side of left, left side of right), where the flexion of the arm is least, to the outside, where it is greatest. This factor, however, is to some slight extent crossed by the tendency to stop on the side of the given point from which it is approached. During the experiments on the left arm, the right hand usually rested near the wrist, or at a point more extended than the point given to be localized. During the experiments on the right arm, however, the left hand usually rested nearer the elbow than the point given. Consequently, in certain of the experiments we find this tendency added to the former on the left arm and subtracted from it on the right, where in some cases it completely nullifies the other.

There is also noticed, in general, some tendency away from the boundaries of the arm when the given point is on the side. During the first experiments, the reagent would occasionally miss the entire arm at the first attempt at localization. Probably fear of this error led him to localize further within than he otherwise would have done. The total results for all series show 196½ errors away from the boundary and 143 towards it for *Wn.*, and 68½ away and 58 towards for *T.* The other reagents show the same tendencies in about the same degree.

All the muscular tendencies noticed in this section are of influence only within the limen. They would, on *à priori* grounds, have no validity outside of the limen. Nor do they seem to determine the amount of the error within the limen. There is no relation noticeable between the tendency to localize in a given direction and an increase of the average error in that direction. An examination of the Tables will show

[1] "On the Perception of Small Differences." *E. g.*, pp. 48, 49.


NORMAL.


PURPOSED
VISUALISATION.

that the error in the direction *P.* is not proportionately larger than the others, in spite of the pronounced tendency to localize in that direction.

5. During the course of the investigation, the results of M. Henri's[1] experiments came to our notice. His reagents localized the impressions upon a photograph of the arm. The same method had already suggested itself to us, and was employed in some series of experiments. We give the results obtained from the reagent *T.* Two series were taken, one with visualization and the other without particular regard to the visual image.

During the experiments the reagent's arm was screened from his view. A point was touched on his arm, and the pressure continued while he localized it upon the photograph in front of him. The points experimented upon were approximately those used in the previous investigation. They were accurately marked and the same points used for each series. In order that the reagent should not be influenced by a knowledge of the schema, the photographs were used in such an order that the plan did not appear on any of them.

We give the results for the two series collected on single photographs. In the results it will be noticed that the error is in general much greater than in the former experiments, and the difference between the upper parts of the arm and the area marked by the folds of the wrist joint is much greater. This increase of the error is due, in all probability, to the lack of the correcting influence of the local signs present in the other method as the skin is touched in the search for the original stimulus. Here the first translation into visual space must suffice; there can be no further aid received from the new contacts. The folds of the skin at the joint, however, are prominent landmarks, to which the point can be accurately referred, and by whose aid it can be correctly localized. The cords are not so prominent in the photograph as on the skin itself, but in any case would only aid in fixing the point on its vertical line. The increased tendency to localize towards the wrist is in part due to an estimation of the distance in terms of the portion of the wrist usually exposed, without consideration of the increased surface offered by the turning back of the sleeve. Besides this, the tendencies noticed as effective in the other problem are at work with increased effect, since the restraining influence of the pure tactual limen of twoness is to a large extent lost.

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[1] *Récherches sur la localisation des sensations. Archives de physiologie*, No. 4, 1893, 619-627.

The increased power of localization, when the subject is visualizing, is very striking. Here the translation into visual terms before attempting to localize is more complete, and the subject is much better prepared to perform the localization with something like visual exactness.

On the whole, however, it seems that the errors avoided by the method are not so great as is the new one introduced. The problem changes its form to a certain extent, and becomes a matter of estimating the distance of a known point from different landmarks and then recording this estimation by a second estimation of distances in a slightly different field.

Postscript.—Since the above was written, Lewy's[1] article on memory has appeared. In this, use is made of the same method to test the memory of localization for both normal and pathological subjects, in the hope of discovering a method of diagnosing mental diseases. The results agree with our own so far as they cover the same ground. There is found the same marked displacement towards the wrist in all subjects. Lewy seems to incline towards an explanation in terms of the local signs, but does not attempt to give a full explanation. There is found the variation in the size of the error with the part of the arm experimented upon. This, however, is successfully avoided by using a very small portion of the arm. Fechner's method of mean error is used in interpreting the results without any compensation made for the error peculiar to this problem: that the average error is always too small. The various factors (visual, motor and tactual) that affect the memory do not seem to be well controlled in the experiments.

Summary.

1. Weber's second method gives a valid measure of the limen after the proper correction has been applied to the average error of localization.

2. Every tactual impression is a compound. This can only be separated into its elements after much practice, and by persons of strong powers of attention and introspection.

3. The change of direction in the long axis of the limen, as one proceeds from upper arm to wrist, is probably due to the visual image connected with the pressure sensation proper.

4. The tendency to make an error towards the wrist is probably due to the overestimation of the extent of muscular movements of flexion as compared with movements of extension.

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[1] *Experimentelle Untersuchungen über das Gedächtniss. Zeitsch. f. Psych. u. Phys. d. Sinnesorg.*, VIII, pp. 231 ff.

5. Localization on a photograph, while giving some interesting results on other points, does not provide an accurate means of determining the limen of twoness.

X.—On the Affective Tone of Simple Sense-Impressions.

By D. R. Major, B. S.

The aim of this study was largely methodological. We wished to test the validity of the serial method, already employed in æsthetics by Witmer,[1] and given by Külpe as one of the two principal methods of affective investigation.[2]

Our experiments were carried out in the months January to June, 1894. Just as we had prepared our material for publication, we received Vol. X., Pt. 4, of the *Philosophische Studien*, in which is contained J. Cohn's paper: *Experimentelle Untersuchungen über die Gefühlsbetonung der Farben, Helligkeiten, und ihrer Combinationen.* It seemed better to defer publication till we had compared the two sets of results. In what follows, therefore, constant reference will be made to Cohn's article.

Methodological.—Külpe writes of the serial method as follows: "Sie geht von der Thatsache aus, dass, wenn auch absolut die Gefühlswirkung eines Reizes unter verschiedenen Umständen sehr verschieden ausfällt, immerhin die einzelnen Reize das Gefühl in constanter Form *relativ* bestimmen oder beeinflussen können. Wenn ich z. B. in der Stimmung bin alle Farben gleichgiltig zu finden, so werde ich dennoch unter einer Anzahl mir vorgelegter Farbentöne Unterschiede ihrer Wirkung auf mein Gefühlsleben anzugeben im Stande sein . . . . . Der Vorzug oder die Zurücksetzung, die den Werthen innerhalb einer solchen Reihe zu Theil werden, geben uns dann einen Aufschluss über die Abhängigkeit des Gefühls von den Reizen . . . . . Eine strengere Ausbildung der Methode hat noch nicht stattgefunden . . . . . Bei [ihr] ist offenbar die Abhängigkeit der Gefühle von den Reizen im allgemeinen nur durch eine Curve darstellbar, deren Verlauf die subjectiven Aenderungen zur Anschauung bringt, die einer bestimmten Aenderungsform der Reize entsprechen. Die einzelnen Werthe dieser Curve haben keine absolute Bedeutung, sondern sind lediglich abgestuft zu denken vom relativ Unangenehmsten bis zum relativ Angenehmsten . . . . Wir können uns die Gefühlsänderung selbst als einen rein

[1] *Phil. Stud.*, IX., pp. 209 ff.
[2] *Grundriss*, p. 239.

quantitativen Process vorstellen, in dem das relativ Unangenehmste ein Minimum, das relativ Angenehmste ein Maximum bildet. Alle Zwischenstufen zwischen diesen Grenzwerthen würden eine stetige Verbindung zwischen ihnen herstellen . . . . . ."[1]

Cohn (1) recommends the "gründliche Heranziehung einzelner Personen" (p. 564). Our experience coincides with his. We confined ourselves to the examination of three subjects: Miss Carss *(C.)*, Miss Hunt *(H.)*, and Mr. Lighty *(L.)*. (2) He declares the serial method inapplicable to colors, for two reasons. In the first place, the qualities contrast with one another, so that a different impression follows from each particular presentation of a series. Secondly, the separate colors "operate not as members of a continuous series, but as independent qualities." Both objections are true: but it is a question whether one should give the method up altogether on their account. We endeavored to meet them, as follows.

I.

To take visual impressions first:—there is the initial difficulty of a color and brightness standard. Cohn, in face of this difficulty, constructed a color circle.[2] "Man sieht . . . wie roh und unexact dieser Farbenkreis ist. Im Folgenden wird sich zeigen, wie gute Dienste er trotz alledem leisten kann." We made no attempt at a new standard, but simply cast about for a convenient series of colored and bright surfaces. Such a series we found in the Bradley Educational Colored Papers (dead finish).[3] The "spectrum scales" of the M. Bradley Co. contain ninety-five saturations; nineteen fully saturated or typical colors, and, for each of these, two less saturated qualities, obtained by the intermixture of different degrees of black, and two obtained by the intermixture of white. The company kindly sent us a number of unclassified additional papers, which we arranged like the rest, as carefully and accurately as possible. We thus obtained a total of 137 color qualities. Besides these, we had twelve qualities from those of the brighness scale.

The first objection to the serial method is the influence of contrast. This renders it impossible to present a series of colors simultaneously. To avoid it, we exhibited but one visual quality at a time. Squares of 6 cm. were cut from the

[1] *Op. cit.*, pp. 239-242, 254, 257, 260. For the method as employed by Witmer, cf. *Phil. Stud.*, X., p. 563.

[2] Pp. 566-569.

[3] See *The Bradley Color Scheme*, p. 6; *Bradley's School Aids*, pp. 1, 2.

stimulus papers. These were exposed, in two different experimental series, upon cardboard backgrounds of white and black respectively. The cardboard in each case was 60 cm. square. Behind it, at the distance of about 1 m., was a wall of neutral yellow-grey. The observer sat, with closed eyes, 2 m. from the card upon which the stimulus paper was exposed. The signal for attention was given 2″ before exposure; the exposure was made for 2″; and some 10″ were allowed for the recording of the affective judgment. Often less time was needed for this last.

This method of isolated exposure constitutes, of course, a radical modification of the serial method. Does it involve the serial method at all? And does it get rid, as it was intended to do, of contrast effects?

The answer to the first of these questions leads us to the second objection: that the colors operate as independent qualities. In our experiments they certainly did not. Our plan was: to begin the experimental series at any point in the spectrum; to move always, having begun, from right to left along the spectral series; to set out either from the whitest or from the blackest saturation of the color chosen as the first stimulus, and to present all the saturation degrees of this color before passing to the next.—then to take the saturation degrees of this next color in the same order,—and so on; and to give the whole series of 137 stimuli at each sitting. Thus the observers, who were familiar with the solar spectrum, knew the spectral direction of the series, and could hold the total scheme more or less definitely in memory; while at the same time the number of saturation degrees of each color afforded a sufficient break between color and color. The beginning of the total series at any point of the spectrum was a further correction of possible *Einstellung*. Again, not only were the impressions held together serially by aid of the memorial spectral scheme, but each separate group of saturation degrees of a single color formed a minor series in itself, from term to term of which the affective judgment might vary considerably. As stated above, the order of exposure of saturations might be either from dark to light or from light to dark; but it was constant within the same series, and therefore known to and expected by the observer after the first few impressions had been given.

On this method, contrast, if present at all, would plainly tend to be not contrast of color with color, but contrast of *e. g.*, the brightest saturation of one color with the darkest saturation of the next following. But of any such contrast effect there is no trace in the experimental results. Partly, it may be, the time interval between experiment and experi-

ment was long enough, and the time of exposure short enough, to prevent it; partly, the observers were on their guard against allowing expectation to pass over into comparison; partly, it seemed that, at the conclusion of each minor series of saturation degrees, this series was replaced in mind by the thought of the total spectral series—the expectant attention turned to the next color in general, and not to a particular saturation degree of it, so that, again, a direct mental comparison was avoided.

Of course, with either background, there was induction. And the error so introduced, though purporting to be constant, was not so in reality, since the brightness of the six "principal" colors in the Bradley scheme (as in the spectrum) are by no means the same. But it seemed unavoidable. Some trial experiments were made in the winter of 1893 with large colored surfaces. In every case the observer expressed himself as feeling "discomfort" in the presence of the extended stimulus, and was convinced that judgment would be more prompt and certain if the stimulus surface was reduced in size. Moreover, the extended color exhausts the retina, leaving the after-effects of exhaustion behind it. It is noteworthy that incidental remarks to this effect were dropped by the observers *C.* and *H.* during the present experiments.

(3) Cohn attributes to the serial method a "fundamental error." It is inadequate. "Sie kann nur einen oder den andern Hauptpunkt der Curve, nicht die Curve selbst feststellen." This is true of the method in its usual form. To obviate the difficulty, we formed an arbitrary scale of affective values. It was found, after a good deal of testing, that seven such values could be readily held in memory. They were: 1, very pleasant; 2, moderately pleasant; 3, just pleasant; 4, without affective tone; 5, just unpleasant; 6, moderately unpleasant; 7, very unpleasant. This scale was written upon a blackboard in the experimenting room; so that the observers could refer to it before experimentation began, and refresh their memory by casting a glance at it between experiment and experiment, before closing the eyes. If we take 4 as the abscissa of the affective curve, it is clear that 1, 2 and 3 may be regarded as positive; 5, 6 and 7 as negative ordinates.[1] We do not mean, of course, that there is anything absolute in the curve thus obtained. It was very quickly noticed, during our experiments with visual impressions, that the observers were not accustomed to attribute the terms "pleasant" and

[1] This procedure is not that criticised by Külpe, p. 241.

"unpleasant" to visual stimuli of the kind investigated. Hence, when it was required of them to predicate these terms, they attached individual meanings to them. *L.* persisted throughout in regarding as visually unpleasant only a stimulus which was absolutely injurious, destructive to the organ, such as direct sun-light. His curves, therefore, show hardly any negative ordinates. Often he described a color as "ugly"; but did not on that account find it "unpleasant." For *C.* and *H.*, on the contrary, "ugly" and "unpleasant" meant, so far as we could tell, the same thing. The curves of *L.* and of *C.* and *H.* cannot on this account be compared with one another. Nor do we claim anything but relativity of result within the limits of the same curve. That 1 is so high above 4, absolutely, as 7 is below it, we have no means of knowing. What we do maintain, however, is that in these seven affective values, we have a sliding scale which is easily memorized, which assists the observer in the formation of an affective judgment, which is applicable to the series of impressions whatever the mood *(Stimmung)* of the observer at the time of experimentation may be, and which removes in a relatively trustworthy way the "fundamental" objection of the inadequacy of the serial method. The results shall speak for themselves later. It should be mentioned that to eliminate the direct memory error, we worked only once a week.

The Affective Judgment.—Our notes on this head practically coincide with those of Cohn (pp. 596 ff.). We noticed the difficulty of obtaining the "gleichmässige Gemüthslage" from the observers. This is partly due to the causes enumerated by Cohn (the confusion of the æsthetic with the affective judgment, the conviction of the "subjectivity" and uncertainty of the experiment, etc.); partly to a cause which we have mentioned above, the unaccustomedness of the required judgment. We found, again, the tendency to theorize; to put reflection in the place of direct experiencing. We were not able, however, like Cohn, to treat our observers' suggestions with "cool scepticism," at the time of their utterance. This would have meant a discontented mood and variable judgment on their part. We treated the suggestions with some respect; but discouraged their making during an experimental series. In other words, instead of discouraging introspection altogether, as Cohn did, we worked towards mechanization during the experimental hour, but allowed the observer to introspect memorially at its conclusion. Many of the suggestions could be discounted at once; but the observer — ignorant of the actual aim of the investigation — was flattered and put into a good mood by being able to discuss

them, and see them noted down. The point is a difficult one; and every observer must be to some extent individually handled with regard to it.

We noticed, like Cohn, that the number of distracting associations decreased as the experiments progressed. This we ascribe partly to the serial nature of the impressions (Witmer), partly to the number of experiments made (Cohn). There were, altogether, but few associations. The observer *L.* remarked that it was generally possible to judge of the color as color; but that if once a train of association was allowed to start, it was liable to persist. This latter statement was confirmed by the other two observers. Greens seemed for all three observers to be the richest in associations. It was hard to discover the reason: *C.* suggested the green of vegetation, *L.* silk dresses. There were three main types of association: (*a*) with natural objects—flowers, birds, etc.; (*b*) with moods (Cohn, p. 590); (*c*) with objects of analogous attributes, delicate tones being associated to fragile fabrics, etc. (Cohn, pp. 565, 598.)

As regards the effects of mood, the utterances of our observers differ somewhat from those of Cohn. He found that a particular mood might condition a particular judgment; and that a good mood meant a better cognition of affective differences. We obtained no instance under the first rubric: that may be simple accident. As regards the second point, we noticed that "good spirits," a pleasant day after much wet weather, etc., so far from emphasizing the differences between the pleasant and unpleasant impressions, sent all impressions alike up in the direction of pleasantness. Mood appears to be a factor which varies with different individuals.

Two of Cohn's observers remarked on occasion that the order of experimentation influenced judgment. One of our own *(H.)* made a similar remark two or three times. But the remark was made as an "impression" at the conclusion of the experimental series; and no trace of any such influence could be found in the observer's results. Such unreliable "impressions" are especially apt to arise in unfavorable moods. In certain cases the direct result of stimulation of the sense-organ determined the affective judgment. "Cool," "restful" colors were pleasant; dazzling, blinding, "angry" colors unpleasant. The cases are not numerous.

Results of Experiments with Visual Impressions.—We proceed to give in Table I the results of experiments upon our 137 saturations. The spectrum is divided into twenty-seven qualities, described as: between violet-red and red-violet; violet-red; red; orange-red; red-orange; orange; yellow-orange;

orange-yellow; yellow; green-yellow; between green-yellow and yellow-green; yellow-green; green; green-blue-green; blue-green; between blue-green and green blue; green-blue; extra green-blue; slate blue; blue; violet-blue; blue-violet; slate violet; violet; extra red-violet; extra violet-red; red-violet. The nomenclature is, as has been indicated, to a certain extent arbitrary. For each of these qualities there is a principal quality or most saturated color; (this is placed in the curve upon the vertical line indicated above by its initial letter or letters, and below by asterisks;) and a number of less saturated qualities, dark and light. The order in the curve is in each case from light to dark. The total number of saturations, besides the normal color in each minor series, is: (1) 2 light and 3 dark; (2) 2 and 3; (3), (4) 2 and 2; (5) 3 and 2; (6) 4 and 2; (7) 3 and 3; (8) 2 and 3; (9), (10) 2 and 2; (11) 2 dark only; (12) 2 and 2; (13) 2 and 3; (14) 1 and 3; (15) 2 and 2; (16) 1 and 1; (17), (18) 2 and 2; (19) 1 and 1; (20), (21), (22) 2 and 2; (23) 2 dark only; (24), (25) 2 and 2; (26) 2 and 3; (27) 2 and 2,—in all, as stated, 137. It may be noted that while the normal series for each color contained five qualities, there was sufficient irregularity to prevent any exact definitizing of the attention:—another reason for the absence of contrast effects in the results.

The figures in vertical lines to the left of the curves represent the affective judgments; 1 = very pleasant, 7 = very unpleasant.

The first letter above each curve gives the name of the observer. The seventh curve (*W.*) was taken from a subject not hitherto named, Miss L. Washburn, who was only present during the first part of the investigation.

The second letter gives the background; *W.* = white, *B.* = black.

The figure following shows the number of times that affective judgments were recorded by the observer for the whole series of 137 saturations, apart from practice experiments.

All experiments were made in diffuse daylight, under constant conditions of illumination.

Several questions arise in connection with this table.

(*a*) Was the affective judgment constant? And if so, within what limits? It would appear to be a defect of the serial method in the form in which we have employed it, that but few judgments of the same stimulus can be taken. Thus although *C.*, *e. g.*, was the subject of 1,233 experiments with the black background, each particular saturation was affectively estimated only nine times. It might be expected that this would mean a very large *m. v.* But the results give the con-

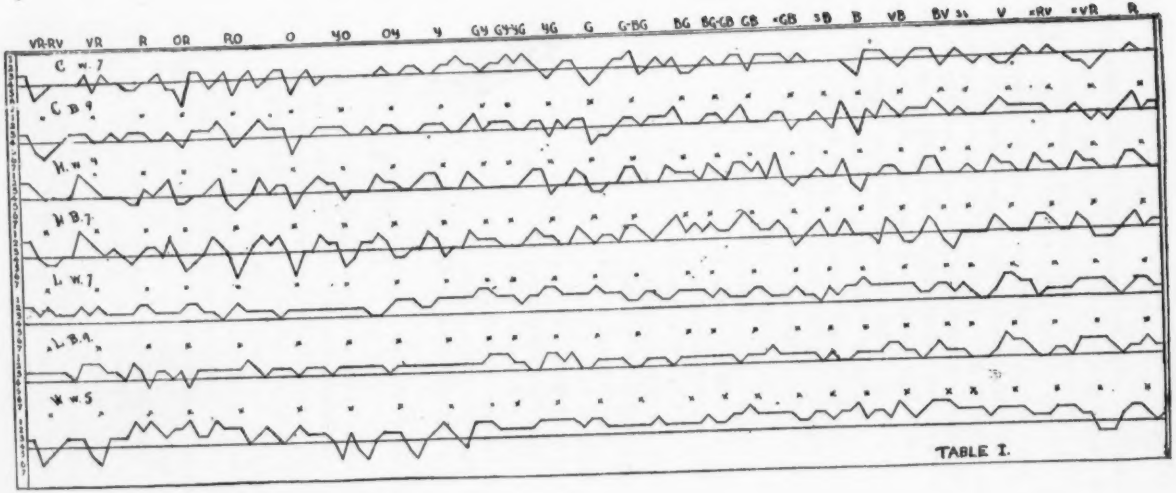

VR-RV VR R OR RO O YO OY Y GY GY-YG YG G G-BG BG BG-GB GB -GB BB B VB BV V -RV -VR R
C. W. ♀
C. B. ♀
H. W. ♀
H. B. ♂
L. W. ♂
L. B. A.
W. W. S

TABLE I.

trary verdict. On the average, for each individual, there was a quite unexpected constancy of judgment.

It would be easy to prove this constancy by giving the *m. v.* for each of the 137 saturations and for each observer. But, apart from the cumbrousness of such a method, the table constructed would be to some extent misleading. First, the judgment-series present several well-marked types,—which could not be differentiated; while, secondly, the same *m. v.* would have a very different psychological value, according as it was a variation within one and the same affective quality, or between the two, of pleasantness and unpleasantness. We will, therefore, rather enumerate the types of judgment-series obtained, together with actual illustrations of them, these illustrations serving to indicate the range of the *m. v.*

i. *Perfectly regular affective series.* *L.*; 1, 1, 1, 1, 1, 1, 1, 1, 1. *H.*; 3, 3, 3, 3; 5, 5, 5, 5, 5, 5, 5. *C.*; 3, 3, 3, 3, 3, 3, 3, 3, 3. *W.*; 2, 2, 2, 2, 2. These series were naturally rare; although considerably more occurred than we had looked for.

ii. *Affective series becoming regular as experimentation progressed.* *C.*; 5, 5, 3, 3, 3, 3, 3. *L.*; 4, 3, 3, 3, 3, 3, 3. These were more numerous.

iii. *Series varying within the same affective quality.* *C.*; 5, 5, 6, 6, 7, 6, 6: or 5, 6, 6, 6, 5, 6, 6, 5, 5. *L.*; 2, 2, 2, 1, 1, 3, 3, 3, 2. *H.*; 7, 5, 7, 6. *W.*; 5, 7, 5, 5, 6. These were very numerous indeed.

[Cross-series between ii. and iii. were common. Thus: *C.*; 2, 5, 5, 6, 5, 5, 5. *H.*; 3, 2, 2, 2, 2, 1, 2.]

iv. *Regular series abruptly broken by a single term.* *C.*; 5, 5, 5, 5, 3, 5, 5; 3, 3, 3, 4, 3, 3. *H.*; 3, 3, 2, 3; 4, 4, 2, 4.

[The pure type is not very common. Quite frequent, on the other hand, are cross-series, involving iv. and iii., iv. and ii., or all three types. Thus: *C.*; 4, 5, 5, 5, 3, 5, 5=ii. and iv.; *C.*; 1, 2, 3, 3, 4, 3, 3, 3, 3=ii. and iv.; *C.*; 5, 3, 6, 5, 6, 5, 5=iii. and iv.; so *H.*; 5, 5, 3, 6, 5, 5, 5; and *L.*; 5, 5, 6, 5, 5, 3, 5; or 3, 7, 2, 2, 2, 3, 3; *L.*; 2, 3, 3, 3, 3, 5, 3, 3, 3=all three; etc.]

v. *Progressive and regressive affective series.* These are very rare; and cannot, we think, be referred to any but variable influences. We have only the following out of the total number of series: *L.*; 2, 3, 3, 3, 3, 3, 5, 5, 5: *H.*; 1, 3, 3, 3, 3, 5, 5 (this may really be a combination of ii. and vi.): *C.*; 5, 5, 5, 5, 6, 5, 6, 7, 7 (this is a cross-series of iii. and v.): *H.*; 5, 7, 5, 5, 2, 2, 2 and 5, 7, 7, 5, 3, 3, 3 (iii. and v.); *cf.* *L.*; 6, 4, 3, 1, 2, 2, 2. These six or five cases, taken together with a very few doubtful

ones, seem to require the rubric. We can understand that familiarity should make an impression pleasanter; but it is difficult to understand why one should become less pleasant,—unless acquaintance with so large a color series gave the subject a richer mental furniture, and made him or her more critical as experimentation progressed. In neither case do we imagine recognition to have taken place.

vi. *Series of indifference.* By our figure 4 we do not, of course, understand a new affective quality, beyond those of pleasantness and unpleasantness; the judgment "no affective tone" corresponds to it. We can here distinguish sub-types:

(*a*) *Regular non-affective series.* *H.*; 4, 4, 4, 4, 4, 4, 4; so *C.*—*L.* has 4, 4, 4, 4, 4, 1, 4 (complication of vi. *a* with iv.). The type is fairly common with *H.*; else rare.

(*b*) *Vacillations round the non-affective judgment.* *C.*; 5, 5, 3, 5, 3, 3, 3; a series whose type frequently recurs in *C.*'s results. *H.*, 3, 3, 5, 5, 5, 3, 3; etc. Besides these, we have series in which the 4-judgment itself occurs. *H.*; 4, 5, 5, 4, 3, 3, 3; or 3, 3, 4, 4, 4, 4, 4 (*cf.* ii.). *C.*; 4, 3, 4, 3, 4, 4, 5, 5, 4. *L.*; 3, 4, 4, 4, 3, 5, 4, 5, 4. [Cross-series were again frequent.] With these, *cf.* the *Gleichheitsurtheile* mentioned by Cohn, p. 598.

vii. *Irregular series.* These are very rare; most seemingly irregular series reducing themselves to order on a close inspection. We find: *C.*; 3, 2, 6, 5, 3, 3, 4; 5, 3, 3, 6, 7, 5, 5; *L.*; 2, 3, 1, 4, 5, 3, 5; *H.*; 3, 7, 6, 3, 6, 3, 3;—but these are the only instances of really irregular series; and even of them, the second and third are perhaps regressive. Such series as: *L.*; 6, 4, 3, 1, 2, 2, 2 show (as remarked above) a distinct affective tendency. Again, we have from *C.* the series: 2, 2, 6, 7, 6, 3, 3,—but notes on the observer's record-cards explained the three high figures as due to a particular influence.

These statements must not be misunderstood. The results are by no means artificially regular. Types i. and vi. *a* are rare, as stated. Nearly every series shows irregularity of some sort. But the variation is either by way of an isolated interruption (type iv.), due to mood, etc., or lies within one and the same affective quality (type iii.; extremes 1 and 3, or 5 and 7). In no other instance than those mentioned is there a variation from 7 to 3, 6 to 2, or 5 to 1, which is not referable to one of the above rubrics. We ascribe this regularity partly to the unaccustomed nature of the required judgments,

—the observers tended to move cautiously within the limits prescribed for them, so that the values 5 to 2, inclusive, predominate in the records; partly to the mechanization of the judgment,—a point emphasized by Cohn; and partly to the taking up of a definite affective attitude toward particular saturation types. Of this last, more presently.

Plainly, the giving of the *m. v.* of these results would be useless, and even misleading. The results require careful individual evaluation.

(*b*) Is a greater or less degree of saturation on the average more pleasant? Cohn writes: "Von zwei Nüancen derselben Farbe gefällt die gesättigtere besser. Auch unter einer Reihe verschiedener Farber werden im algemeinen die gesättigteren bevorzugt" (p. 511). A glance at the first four curves of our Table I. will show that for the observers *C.* and *H.*, precisely the opposite holds. There is generally an "absolute" unpleasantness attaching to the more saturated colors; nearly always a relative unpleasantness. The curves are exceedingly regular in this respect. For *L.* it is different. The most saturated colors usually coincide with high ordinates throughout his first curve; and with fairly high ones in the second part of the second. In the first half of the latter, however, some of the most saturated colors coincide with minima of pleasantness. *W.*'s curve varies in this respect. At the beginning and end, the curve drops for saturated impressions; but over the greater part of its course it rises to them, or remains stationary when they occur.

Cohn's rule, then, is not universal (p. 600). There are great individual differences. When one thinks how dependent the sense-feeling is upon central excitations, that is not to be wondered at.—Of course, Cohn used a different method from ours, and employed principally gelatine plates and rotating discs where we had colored papers. Still, this cannot of itself account for the discrepancy.

Cohn continues: "Unter annähernd gleichgesättigten Farben scheint die Bevorzugung auf rein individuellen Neigungen zu beruhen. Nur das Gelbe dürfte für die Mehrzahl hinter den anderen Farben zurückstehen, auch wenn es ganz gesättigt ist." Taking our own most saturated colors, *R*, *O*, *Y*, *G*, *B*, *V*,—although we do not mean to imply that their saturations are absolutely equal,—we find that:

(i.) *C.* makes *O*, *G* and *B* distinctly unpleasant; *R*, *Y* and *V* just pleasant, just unpleasant, or indifferent.

(ii.) *H.* makes *O* distinctly unpleasant; *B* rather so; *R* just pleasant; *G*, *Y* just unpleasant or indifferent; *V* just pleasant or indifferent.

(iii.) *L.* makes the right hand of the spectrum more pleasant than the left. *R* alone falls below the abscissa of "no affective tone."

(iv.) *W.* makes *R* the most, *B* and *Y* the least pleasant colors. All are pleasant.

Here we have full evidence of the "individuelle Neigungen;" but none of a dislike of yellow. Yellow is just pleasant for *C.*; just unpleasant or not toned for *H.*; moderately or just pleasant for *L.*; just pleasant for *W.* Cohn's caution as to the insufficiency of his own results is, therefore, well in place.

(*c*) We may examine this same question, of the relative pleasantness of saturation degrees, in connection with the further question of the influence of the background. If we take the twenty-seven best saturations of our color series, and count the no-tone judgments half to pleasantness and half to unpleasantness, we find that:

C.	on	white	makes	8.5	pleasants,	18.5	unpleasants.
"	"	black	"	13.5	"	13.5	"
H.	"	white	"	15.5	"	11.5	"
"	"	black	"	12.5	"	14.5	"
L.	"	white	"	27.0	"	0.0	"
"	"	black	"	23.5	"	3.5	"
W.	"	white	"	23.5	"	3.5	"

Even this table, which throws together saturation-values of very considerable difference, indicates, when its values are compared with the total numbers of "pleasant" and "unpleasant" judgments (their totals being similarly obtained, by halving the no-tone judgments), that Cohn's rule of the superior pleasantness of the saturated color is not universal. We find that, in all:

C.	on	white	makes	77.5	pleasants,	59.5	unpleasants.
"	"	black	"	98.0	"	39.0	"
H.	"	white	"	91.5	"	45.5	"
"	"	black	"	94.0	"	43.0	"
L.	"	white	"	132.5	"	4.5	"
"	"	black	"	123.0	"	14.0	"
W.	"	white	"	120.5	"	16.5	"

In other words:

C.	on	white	makes,	of all,	58%	pleasant;	of	the	27,	31%
"	"	black	"	"	71%	"	"	"	"	50%
H.	"	white	"	"	66%	"	"	"	"	57%
"	"	black	"	"	68%	"	"	"	"	46%
L.	"	white	"	"	96%	"	"	"	"	100%
"	"	black	"	"	90%	"	"	"	"	87%
W.	"	white	"	"	88%	"	"	"	"	87%

So much, then, for Cohn's generalization. We turn now to the second question: that of the influence of the background.

If the members of each of the three first pairs of curves are compared with one another, it will be seen that the type of each pair is constant; 1 and 2 are alike, 3 and 4, 5 and 6. This fact, we may remark incidentally, tells distinctly in favor of the method employed.

Comparing the curves of each pair in detail, we obtain the following results: (i.) In neither curve does *C.* pass the affective value 2; in one case only does she sink to the value 7 (white, *O R*). White has, as we have seen, 13% less pleasant judgments than black. Black, too, has 19% more of the best saturations pleasant than white. White has, in all, 35 no-tone judgments; black 38. On the average, then, the saturations were a little pleasanter on black than on white. In particular: from the beginning to *R*, black is pleasanter; from *R* to *O*, black; from *O* to *Y*, black; from *Y* to *G*, white; from *G* to *B*, both are practically equal,—black having the advantage in the height of the pleasure ordinates; from *B* to *V*, black is pleasanter; from *V* to the end, black. (ii.) *H.* reaches the value 1 twice, on white; three times on black, she sinks to 7 twice, on black. Black has 2% more of pleasants than white. The best saturations have 11% less pleasants, however, on black. White has, in all, 57 no-tone judgments; black 33. Neither background, it is plain, can be said to be more or less pleasant than the other for this observer. Black, however, called out more affective judgments, and shows more extremes, both of pleasantness and unpleasantness, than white. In detail: from the beginning to *R*, white is slightly pleasanter; from *R* to *O* there is practical equality; from *O* to *Y*, again; from *Y* to G, black is pleasanter; from *G* to *B*, black; from *B* to *V*, white (the effect of black is to "impair" these colors, the observer stated); from *V* to the end, there is equality. (iii.) *L.* gives the value 1 eight times on white, once only on black. No impression is unpleasant on white; two are, on black. White has 6% more pleasants than black; the best saturations on white, 13%. White has in all 9 no-tone judgments; black 26. The white background, then, is obviously the pleasanter. The observer himself recorded this fact only in regard to the blues. White is pleasanter for every section of the curve; particularly, however, between *O Y* and *B V*. (iv.) It is unfortunate that we have no black curve to compare with the last one of the table. This curve is particularly well marked: there is distinct dislike, *e. g.*, of the oranges and purples on a white ground. Such a regional dislike is not manifested by the other

observers. *L.* hints at it, for the reds on black. In the other curves, pleasants and unpleasants alternate in more individual preference.—*Cf.* §4 of Cohn's *Results*, p. 600.

(*d*) Are dark or light saturations the pleasanter? If we take the "shades" (dark saturations) and "tints" (light saturations) of the twenty-five qualities (omitting the slate-blue and the slate-violet), we find that, on white:

C.	makes	7	shades	and	5	tints	pleasanter:	13	no-preferences.
H.	"	2	"	"	23	"	"	no	"	"
L.	"	5	"	"	7	"	"	13	"	"
W.	"	6	"	"	14	"	"	5	"	"

Here are two types, evidently: *C.* and *L.* have no preference, while *H.* and *W.* distinctly prefer the brighter saturations. Further, on black:

C.	makes	1	shade	and	21	tints	pleasanter:	3	no-preferences.
H.	"	1	"	"	24	"	"	no	"	"
L.	"	7	"	"	5	"	"	13	"	"

Here are the same two types. *C.* has gone over to the other side; she and *H.* find the brighter saturations distinctly pleasanter. *L.* continues to show no preference. *C.* noted this change of her affective judgment more than once on her record card.

There is no hint of a preference for the darker saturations in the results of any of the observers.

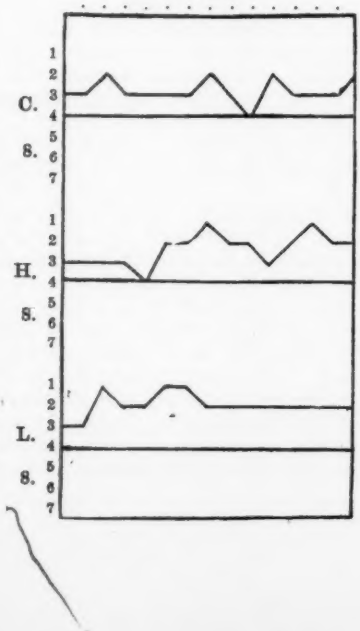
(*e*) Experiments upon the affective tones of the qualities of the brightness-scale were made upon both backgrounds. No brightness-impression is, probably, entirely colorless. Adopting the Bradley nomenclature, we had: a slightly greenish grey, three saturations of "grey," two of "cool-grey," two of "neutral grey," and two of "warm grey," besides white and black. Judgments were taken from *C.* 8, from *H.* 7, and from *L.* 8 times. (*a*) Of the eleven judgments on black, *C.* has 8 marked "no-tone," 3 "pleasant" (values 2, 3, 3); *H.* has 2 marked "no-tone," 8 "pleasant" (seven 3's and one 2), and 1 "unpleasant" (5); while *L.* has 2 marked "no-tone," 7 "pleasant" (3), and 1 "unpleasant" (5). The two unpleasants are white and green-grey. On the whole, then, the qualities on black are just pleasant or indifferent. The individual differences are curious. White gets 3 from *C.* and *L.*, but 5 from *H.* The latter figure tells against Cohn's law of contrast (p. 600). Green-grey gets 2 from *H.* and *C.*, while *L.* gives it 5. This grey, then, is pleasanter than white to *C.* and *H.*, on the black background; while *L.* finds 8 qualities pleasanter than it, of which

white is one. The other judgments, 3's and 4's, are irregularly distributed. There is no trace of the preference of white over grey, found by Cohn, except a very equivocal one in *C.*'s figures. (*b*) The judgments on the white ground are all 3's and 4's, irregularly distributed; except that both *C.* and *L.* give a 5 to the green-grey. *W.* took part in this series, with seven sets of judgments. Black is not pleasanter on the white than any of the greys, with the exception mentioned.—The qualities on the whole, then, are just pleasant or indifferent.

II.

Experiments with Pure Tones.—We made a number of experiments with König tuning-forks. Preparation and signal were as before. The fork was then struck a sharp blow with a cork hammer, and judgment made at once, as the tone became full and clear. The fork was damped, so soon as the experimenter observed that all the subjects had realized the affective tone of the stimulus. A compound piano clang was given after each experiment, to prevent the influence of tone by preceding tone. The order of stimulation was quite irregular. The vibration-rates of the forks, in simple vibrations, were: 512, 576, 640, 682, 768, 853, 960, 1,024, 1,250, 1,536, 2,048, 2,304, 2,560, 1,792. In the table the judgments are recorded in this order.

TABLE II.



The observers again remarked on the unaccustomedness of the ascription of an affective tone to the stimulus. The table shows individual differences: *L.*, as before, is more lavish of his "pleasant" judgments than are *C.* and *H.* For *C.*, the e^3 falls from 2 to 3, the e^4 from 3 to no-tone, the e^5 stays at 3. The d^3 and d^5 are differently toned, so the g^3 and g^4. The three *c*'s are constant at 3. For *H.*, the *e*'s are at 3, 2, 2; the *d*'s at 3 and 1; the *g*'s at 2 and 3; the *c*'s at 3, 2, 2. For *L.*, the *e*'s are at 2, 2, 2; the *d*'s at 1, 2; the *g*'s at 1, 2; the *c*'s at 3, 2, 2. We lay no stress upon this regularity or irregularity,—the experiments have not a sufficient range,—upon the fact that for *H.* the higher tones seem more pleasant, or upon the fact that no judgment falls below the value 4. The method can hardly be called "serial"; the serial element has been modified practically to disappearance. We would conclude from the results simply this: (*a*) that the smallness of the *m. v.* (each curve implies eight sets of judgments) shows that the affective judgment, once made, is persistent; (*b*) that individual differences exist here, as for sight, and that these differences are both general (*cf.* the three curves) and particular (*cf. C.*'s attitude to the *e*'s).

III.

Experiments Upon Active Touch.—These experiments were made upon a series of fifty-one textures. The observer sat with closed eyes. At a signal he laid his open right hand upon a rest. A piece of the fabric under consideration was placed between the thumb and index finger by the experimenter; and the observer moved it to and fro, "feeling" and "judging" it for 2″. Then at a further signal the experimenter removed the stimulus, and the subject recorded his affective judgment.

The stuffs employed cannot be qualitatively classified unless at very great length. We have thought it best, therefore, simply to name them. Samples can be supplied by the laboratory, if anyone should wish to repeat the work with the same stimuli. The following list follows the order of the judgments recorded in the curves from left to right: it was the constant order of experimentation.

Linen canvas, open and close; linen toweling, coarse, medium, fine; gingham, thicker and thinner; percaline; India dimity; butterfly cloth; cotton sateen; cotton silk, thinner, thicker, thinner-and-smoother; gingham, thicker, thinner (both thicker than the two previous kinds); wool serge, thin; Paris cachemère; princess duck; challie laine; wool checks, thin, thicker, thicker; cotton plush, thick, thicker, thick soft, thin soft; flannel; velveteen; washing

silk; organdie, rougher, smoother; broadcloth, thicker, thinner; unbleached muslin; chambray, thinner, thicker; wool serge, thick; Irish lawn; cotton silesia; sateen, rougher, smoother; table linen, bleached and unbleached; dress goods; gingham, ribbed; faced velvet, thick, thicker, thin, thickest; gingham, thick and smooth.

A serial arrangement of these stuffs in any way analogous to the spectral arrangement of colors, could not be carried out: there were too many variables. But we kept the minor series together (as we did the saturation grades of the same color), except in the single case of the ginghams, which we separated from one another for methodological reasons. [The two wool serges were extremely unlike.] We wished to see whether this separation exerted any bad influence, or whether the serial nature of the method — contrast being presumably less marked than in optics—would not be maintained by the simple fact of all judgments coming from the same modalities. We regarded this as probable, because whereas the eye has in all likelihood 40,000 to 50,000 qualities, we have involved here simply pressure and temperature from the skin, and the few qualities of the deeper lying sensibilities that go to make up the movement perception. This ought, we imagined, to hold the stimuli together as a series. We give the curves on page 74.

(*a*) The observers noted that it was far more "natural" to attribute affective tone (and particularly unpleasantness) to these stimuli than to those of sight and sound. And the curves support this: they are all three very much alike,—there is no such indication of individual differences as we have found in the other two tables.

(*b*) The affective judgment was constant, and showed types similar to those enumerated in the first part of this paper.

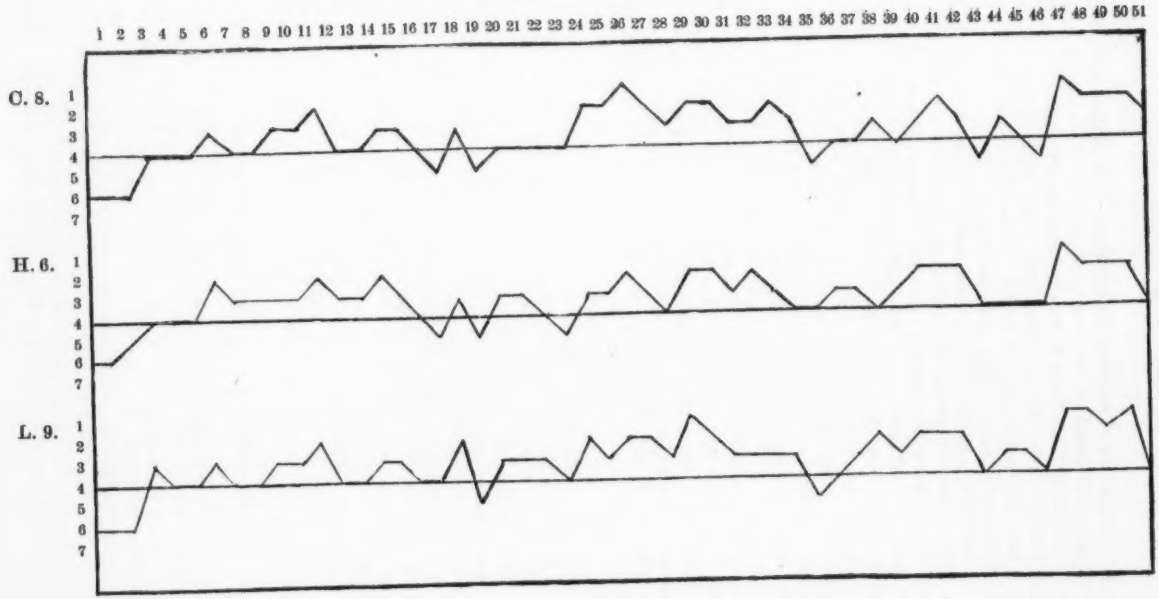
i. Occurs twice only. *C.*; 3, 3, 3, 3, 3, 3, 3, 3; *H.*; 2, 2, 2, 2, 2, 2.

ii. Numerous. *C.*; 1, 2, 1, 1, 1, 1, 1, 1; and 4, 5, 4, 4, 5, 5, 5, 5; etc.

iii. Very numerous. The variation is often between two degrees only. *C.*; 1, 2, 2, 2, 2, 1, 1, 1; *H.*; 3, 2, 3, 3, 2, 3; *L.*; 5, 5, 5, 5, 5, 5, 6, 5, 6; and 3, 2, 3, 2, 2, 2, 1, 2, 2; etc.

iv. Not very common even in the "cross" form. Pure series are: *H.*; 2, 2, 2, 2, 4, 2; and 1, 1, 1, 1, 1, 3; etc. —uncommon. Cross-series are: *C.*; 4, 3, 5, 5, 5, 5, 4, 5 (iv. with ii.); *H.*; 4, 5, 4, 4, 4, 3, 4. We should naturally expect that the interruption would be neither so frequent nor so pronounced in a sense modality in which

TABLE III.


1 2 3 4 5 6 7 8 9 10 11 12 13 14 15 16 17 18 19 20 21 22 23 24 25 26 27 28 29 30 31 32 33 34 35 36 37 38 39 40 41 42 43 44 45 46 47 48 49 50 51
C. 8.
H. 6.
L. 9.
1
2
3
4
5
6
7

the dependence of affective tone upon stimulus is comparatively stable.

v. No commoner than for the colors, as we should, again, for the above reason expect. *C.*; 4, 5, 5, 5, 7, 7, 7, 7; and perhaps the already quoted 1, 2, 2, 2, 2, 1, 1, 1. These seem to be the only instances.

vi. There is no single occurrence of a variation from 7 to 3, 6 to 2, or 5 to 1. This, once more, was to be expected.

(*c*) There is no trace of the influence of contrast in the results. Associations were very few indeed: what there were, were visual,—and usually themselves associatively affective. There was no recognition of the textures from week to week as particular members of the experimental series; though, of course, such stuffs as velvet were known by touch. The stimulus attributes that seemed to the observers to call up the affective judgment were thickness and thinness, coarseness and fineness, roughness and smoothness, stiffness and softness. We can test this by reference to the curves.

First, however, we will look at the judgments made of the scattered ginghams and chambrays. These judgments are numbers 6, 7, 15, 16, 36, 37, 46, 51. We find the values to be:

C.; 3, 4, 3, 4, 4, 4, 5, 3.
H.; 2, 3, 3, 4, 3, 3, 4, 4.
L.; 3, 4, 3, 4, 4, 3, 4, 4.

This constancy, we think, is sufficient to prove our hypothesis correct, when it is considered at what different points of the curves these judgments occur. That we might have been even bolder, methodologically, is shown by the judgments attaching to isolated similars, such as numbers 11 and 41, 42; or 29, 47, 48, 49, 50. We have in these cases:

C.; 2, 2, 3; and 2, 1, 2, 2, 2.
H.; 2, 2, 2; and 2, 1, 2, 2, 2.
L.; 2, 2, 2; and 1, 1, 1, 2, 1.

The absolute likeness of the three curves is no less striking than their relative constancies.

As regards the stimulus qualities, *stiffness* may be predicated especially of numbers 1, 2, 19, 35, 43; *softness* of 11, 18, 30, 38, 40, 41, 42. It will be seen that the curves give high affective values for the latter; low for the former. *Roughness* attaches especially to numbers 1, 12, 13, 14, 17, 20, 35, 46; *smoothness* to 18, 24, 25, 26, 27, 29, 30, 40, 41, 42, 47, 48, 49, 50. It is noticeable, again, that the "roughs" have (on the average) low, the "smooths" high affective

values in all the curves. *Coarseness* is especially characteristic of numbers 1, 2, 19, 35, 43; *fineness* of 9, 30, 31, 32. Though the former is distinctly unpleasant, it will be seen from the curves that the latter does not necessarily carry with it any high degree of pleasantness. Both facts are readily intelligible. Lastly, *thick* were numbers 24, 25, 26, 27, 33, 34, 38, 47, 48, 49, 50; *thin*, 20, 30, 31, 32. Here, too, the affective tone depended on a combination of qualities. Pleasant are 24 to 27, 47 to 50, the thick-smooth series; less pleasant 33, 34 and 38 (thick, but roughish). Again, pleasant is 30 (thin and smooth); less pleasant 31, 32 (thin, but less smooth); still less pleasant 20 (thin and rough).—We do not mean to imply that these eight stimulus qualities are psychological qualities. Psychologically regarded, their differences would be, to a large extent, matters of intensity only.

That thickness or thinness is in itself of little affective weight can be seen from numbers 3, 4, 5 (judged by *C.*, 4, 4, 4; *H.*, 4, 4, 4; *L.*, 3, 4, 4); 21, 22, 23 (*C.*, 4, 4, 4; *H.*, 3, 4, 5; *L.*, 3, 3, 4), and from the series of plushes and velvets. Still, the results show that the observers were right in attributing some influence to this pair of qualities. *H.* said that she preferred thin roughs to thicker; and her judgments are for the former (numbers 12, 13, 14, 20, 46) 3, 3, 2, 4, 4; and for the latter (numbers 1, 17, 35) 6, 5, 4. In the former set, we must remember that 12, 13, 14, though rough, were fine. Indeed, as indicated above, the difficulty of classification of the textures was due to the fact that a single piece usually combined different qualities.

It is difficult to say whether we are here still within the boundaries of psychology, or have crossed the border-line of æsthetics (*cf.* Höffding, *Psychologie in Umrissen*, 1887, p. 286). We incline to the former opinion.

Literature.—We refer the reader, primarily, to the literature section of Cohn's article. General discussions will be found in Wundt, *Vorlesungen*, 2d Ed., pp. 227, 228; *Phys. Psych.*, 4th Ed., ch. x.; Sully, *Human Mind*, II., pp. 49 ff.; Ladd, *Phys. Psych.*, pp. 514 ff.; *Psychology*, pp. 184 ff.; Höffding, *op. cit.*, pp. 285 ff.; etc.

Summary of Results.—(1) We have succeeded in modifying the *serial method* in such a way as to make it applicable to the investigation of the *affective tone of colors*. We publish our modification, however, not in any spirit of dogmatic assertion, but rather with a view to invite criticism both of the method itself and of our analysis of the conscious processes underlying it.

(2) The whole question of the *affective tone of colors* is a

very difficult one. This is due partly to the fact of the unaccustomedness of the affective judgment, partly to that of individual differences, and partly to the fact that the centrally excited influences which meet the incoming stimulus, and which are very strong, differ from time to time in one and the same individual.

(3) We cannot confirm Cohn's rule that the more saturated colors are the pleasanter; *individuals differ* in this respect. Neither can we confirm his generalization that yellow is displeasing to the majority of persons.[1]

(4) No constant affective value can be attached to *black and white backgrounds*. We have found black a little pleasanter, white distinctly pleasanter, and no difference.

(5) As regards preference of *bright and dark saturations*, our observers fall into two groups: one prefers the former, one has no preference. There is no preference for the latter shown.—Here, too, the influence of the backgrounds is individually different.

(6) Experiments with *brightnesses* showed that all alike were just pleasant or not toned. We have no evidence of a preference of the black-white over other brightness contrasts, and no certain evidence of the preference of white over grey.

(7) The few experiments that we made with *pure tones* gave, like those made upon sight, regularity of results for each individual, but differences between individual and individual. In their case, the "modified serial" method ceased to be "serial" at all. The experiments were not continued, as the individual affective judgment, though constant, appeared to be something strained and artificial. This might not be the case with a different method.

(8) We have also found the method, in its modified form, applicable to the sensibilities of *active touch*.

(9) But the *affective tone of active touch* is something far more absolute, *i. e.*, far less dependent upon centrally excited influences, than that of colors and pure tones. The method, therefore, again practically loses its serial nature.

(10) *Stiffness*, *roughness* and *coarseness* of textures were unanimously found unpleasant; *softness* and *smoothness* pleasant. Fineness, thickness and thinness gave variable results.

For other results of detail we must refer the reader to the body of the paper.

[1] It is difficult to see why yellow, unless so bright as to be painful to the sense-organ, should be so disliked. Yellow is the "warm" color of painters (Wundt, *Phys. Psych.*, 4th Ed., I., p. 568); although its "mood" is that of excitement, it has not the angriness of red; we are greatly accustomed to it, in sunlight and artificial light; children prefer it (Preyer, *Seele des Kindes*, pp. 9 ff.).

A PSYCHOPHYSICAL VOCABULARY.

By E. B. Titchener.

Experimental Psychology was in its origin, and has remained to a considerable extent in its development, a German science. One of the preliminaries to its assimilation by the English-speaking student, therefore, is the understanding of German psychological terms. But their understanding does not necessarily carry with it an Englishing of them. Indeed, there are many factors that make against translation.

In the year 1889—having ploughed through the book in the two previous years—I set to work to write out Wundt's *Physiologische Psychologie* in English, in the hope that its difficulties would be more easily overcome when met in this more familiar garb. I found, as a matter of fact, that they became greater. My reading of English psychology had given me no vocabulary adequate to the German text, and, imperfect as my knowledge of German was, I had fallen into the habit of psychologizing in a mixed jargon of the two languages. Although at the time I persevered with the translation, and have ever since endeavored to keep in mind duplicate vocabularies, nothing but the necessity of teaching and lecturing in English could have prevented a *lapsus* into unmixed German or the English-German mixture. To one trained in a German laboratory—as nine-tenths of the younger psychologists of to-day have been—thinking in German upon psychological questions must be easier than thinking either in English or French. A natural consequence is that technically psychological articles are dotted with German words and phrases, which the writer quite correctly assumes will be intelligible to his readers.

But now that there is growing up an American school of psychology, which promises to be only second in importance to the German, and that the number of home-trained students and future teachers is increasing, it becomes imperatively necessary for us to have at our disposal a working outfit of technical terms in our own language. And here English possesses

over German the great advantage that it can set aside such terms without any fear of their having a popular connotation which could give rise to misunderstanding. An 'impulse' is less liable to carry side meanings than a 'drive' would be, and I can 'cognize' with much better technical effect than I can 'know.' At the same time, this facility of technical coinage or usage is liable to abuse, and I suppose that few psychologists would refuse to admit that such abuse has actually occurred.[1]

It is with these considerations in mind that I submit the following list of technical German words with English renderings. The great majority of them are taken from the indices of Wundt's large book and of Külpe's *Grundriss*. I have made use of such works as Ladd's "Elements of Physiological Psychology," Sanford's "Course in Experimental Psychology," the translations of Ziehen's *Physiologische Psychologie* and Wundt's *Vorlesungen*, etc. The medical dictionaries consulted rendered but little assistance. If many of the words included in the list seem too familiar to call for comment, I would ask the reader to remember that the vocabulary is meant to assist beginners; if any difficult terms have been omitted, their omission is unintentional. All the translations of new or unfamiliar words should be looked on as suggestions only. An asterisk prefixed to the German indicates that I am dissatisfied with its proposed English equivalent. Many of the terms are, in my opinion, altogether unnecessary or undesirable; but they occur and must be translated. Bracketing out, which I at first attempted, seemed likely to result in a special list, open to the charge of arbitrariness of selection, unless very full reasons were given; and these are not here in place. Some discussions follow at the conclusion of the paper.

ABHÄNGIGKEIT, dependency.
*ABKLINGEN, fall.
ABLENKUNG, distraction *or* diversion.
ABSTUFUNGSMETHODEN, gradation methods.
ACCORD, chord (*not* clang).
AEHNLICHKEIT, similarity.
* AFFECT, emotion.
AGGLUTINATION, agglutination.

[1] I am strongly of the opinion that the English nomenclature of psychological processes must be in the main of Latin-Greek origin, and not of 'Saxon.' The demand for a 'Saxon terminology' is simply a collateral result of our Germanization. Which of the natural sciences has a 'Saxon terminology?' Certainly not physics or chemistry. Good discussions of this question will be found in Smith's "Student's English Language;" Lects. VIII, §7; IX, §7; and XII, §4.

ALLGEMEINVORSTELLUNG, general (*better, perhaps, than* abstract) idea.
ANGEBORENE VORSTELLUNG, connate (*if the context be modern theory*) *or* innate (*if the reference be to earlier writers*) idea.
*ANKLINGEN, rise.
*ANLAGE, disposition.
*ANSCHAUUNG, perception.
APPERCEPTION, apperception,
APPERCEPTIVE VERBINDUNG, apperceptive connection.
ASSIMILATION, assimilation.
ASSOCIATIVE VERBINDUNG, associative connection.
AUFRECHT-SEHEN, erect *or* reinverted vision.
AUGENAXE, optical axis.
AUGENMASS, visual space-appreciation *or* space discrimination.
AUGENSCHWARZ = *Eigenlicht, q. v.*
AUSDEHNUNG, extension.
AUSDRUCKSBEWEGUNG, expressive movement (*usually in the particular sense of* expression of emotion).
AUSDRUCKSMETHODE, method of expression (*affective*).
AUSFALLSERSCHEINUNG, phenomenon of abrogation.
AUSSCHALTUNG, exclusion.

BAHNUNG, facilitation.
BEDINGUNG, condition.
BEGEHREN, desire.
BEGIERDE, desire (*best general word; must often be qualified by a particular adjective*).
BEGRIFF, concept.
BEKANNTHEITSQUALITÄT, quality *of contents* as known *or* quality of knownness (*probably better than* familiarity).
BEOBACHTUNGSFEHLER, error of observation.
BEREITSCHAFT, preparedness (*sometimes* disposition).
BERÜHRUNGSASSOCIATION, association by contiguity.
BERÜHRUNGSEMPFINDUNG, sensation of contact.
BEZIEHUNGSGESETZ, law of relativity.
BLICKFELD, field of regard.
BLICKLINIE, line of regard (*practically identical with Gesichtslinie*).
BLICKPUNKT, point of regard (= *Fixationspunkt*).

CENTRAL ERREGTE EMPFINDUNG, centrally excited sensation.
COMBINATIONSTON, combination tone.
COMPLICATION, complication.
CONSTANTER FEHLER, constant error.
CONTRACTIONSEMPFINDUNG, sensation of contraction.
CONTROLVERSUCH, test *or* check experiment.

DAUER, duration.
DECKBILD, congruent image (= *Ganzbild*).
DECKPUNKTE, congruent points (*sometimes* = identical *or* corresponding).
DEUTLICHKEIT, distinctness (*for Külpe* = *Klarheit; for Wundt different. See Klarheit below*).
DIFFERENZIRUNG, differentiation.
DIFFERENZTON, difference tone.
DISPOSITION, disposition.
DISSIMILATION, dissimilation.
DOPPELBILD, double image.
DOPPELTE BERÜHRUNGSEMPFINDUNG, sensation of double contact.
DREHPUNKT, centre of rotation.

DREHSCHWINDEL, rotatory vertigo.
DREIKLANG, common chord, triad (*better, as a rule, than* triple clang).
DRUCKEMPFINDUNG, pressure sensation.
DRUCKPUNKT, pressure point *or* spot.
DUR, major.

EBENMERKLICH, just noticeable, *or* liminal.
EIGENLICHT, intrinsic light (*of the retina*) *or* idio-retinal light.
EINBILDUNG, imagination.
EINFACHHEIT, simplicity.
EINKLANG, unison.
*EINSTELLUNG, predisposition.
EINZELKLANG, single clang *or* note.
ELEMENTE DES BEWUSSTSEINS, conscious elements.
EMPFINDEN, sense.
EMPFINDLICHKEIT, sensitivity.
EMPFINDUNG, sensation.
EMPFINDUNGSKREIS, sensation circle.
ENTFERNUNG, distance.
ERFAHRUNG, experience.
ERHEBUNGSWINKEL, angle of vertical displacement *or* ascensional angle *or* angle of elevation.
ERINNERUNG, recollection, remembrance, memory.
ERINNERUNGSBILD, memorial image *or* idea.
ERKENNEN, cognize.
ERLEBNISS, fact *or* datum of experience.
ERMÜDUNGSEMPFINDUNG, sensation of fatigue.
ERREGBARKEIT, excitability.
ERREGEN, excite.
ERREGUNG, excitation.
ERWARTUNG, expectation.

FARBE, color.
FARBENTON, color-tone.
FARBENGRAD, saturation.
FEHLERMETHODEN, error methods.
FEHLREIZ, error stimulus.
FEINHEIT [*der Unterschiedsempfindlichkeit*], delicacy.
FIXATIONSPUNKT, fixation point.
FREI STEIGENDE [*Vorstellungen*], spontaneous.
FUNDAMENTALFORMEL, fundamental formula.
FUNDAMENTALTABELLE, fundamental table.

GANZBILDER, congruent *or* total images (= *Deckbilder*).
GEBERDENSPRACHE, gesture language.
GEDÄCHTNISS, memory.
GEFÜHL, feeling.
GEFÜHLSTON, affection, affective tone *or* coloring.
GEIST (*in modern usage*), mind.
GEISTESSTÖRUNG, mental derangement.
GELENKEMPFINDUNG, articular sensation.
GEMEINEMPFINDUNG, common sensation.
GEMEINGEFÜHL, common feeling.
*GEMÜTH, feeling and will; the sum-total of affective-conative processes.
GEMÜTHSBEWEGUNG, affective *or* affective-conative process *or* condition.
GEMÜTHSVORGANG, affective *or* affective-conative process.

GEMÜTHSZUSTAND, affective *or* affective-conative state.
GERÄUSCH, noise.
GERÄUSCHHÖHE, noise-pitch.
•GESAMMTVORSTELLUNG, composite *or* complex idea; *or* resultant idea.
GESICHTSLINIE, visual axis *or* line of vision.
GESICHTSWINKEL, visual angle.
GEWÖHNUNG, habituation.
GLANZ, luster.
GLEICHGEWICHTSEMPFINDUNG, sensation of equilibrium.
GLEICHHEIT, parity *or* indistinguishableness (*in the doctrine of association; elsewhere often* = *Aehnlichkeit*).
GOLDENER SCHNITT, golden section.
GRÖSSE, magnitude.
GRUNDEMPFINDUNG, primary *or* fundamental sensation.
GRUNDFARBE, primary color.
GRUNDKLANG, fundamental clang *or* tonic clang.
GRUNDTON, fundamental *or* fundamental tone *or* tonic.

HÄUFIGKEITSMETHODE, method of frequency.
HALBBILD, single image.
HANDLUNG, action.
HAPTIK, haptics.
HAUPTFARBE, principal color.
HAUPTRICHTUNGSSTRAHL, principal line of direction (= *Gesichtslinie*).
HAUTEMPFINDUNG, cutaneous sensation.
HAUTSINN, cutaneous sensibility.
HELLIGKEITSEMPFINDUNG, sensation of brightness.
HEMMUNG, inhibition.
HEMMUNGSWIRKUNG, inhibitory effect *or* action.

INDIFFERENZPUNKT, indifference point (*qualitative*).
INDIFFERENZZEIT, natural *or* normal time.
INNERVATIONSEMPFINDUNG, sensation of innervation.
INTELLECTUELLES GEFÜHL, sentiment (= *the so-called* emotion of relation).
INTENSIVE SENSIBILITÄT, intensive sensibility.
INTERMITTENZTON, tone of intermittence.

KÄLTEPUNKT, cold point *or* spot.
KINÆSTHETISCHE EMPFINDUNG, kinæsthetic sensation.
KLANG, clang (*not* chord).
KLANGEINHEIT, clang unity *or* simplicity.
KLANGFARBE, clang-color *or* timbre.
KLANGVERWANDSCHAFT, clang relationship.
KLARHEIT, plainness (*better, probably, than vividness: see Deutlichkeit, above*); clearness.

LAGEEMPFINDUNG, sensation of position.
LEIDENSCHAFT, passion.
LEITTON, leading tone *or* note.
LEITUNG, conduction.
LEITUNGSBAHN, conduction path; tract.
LICHTEMPFINDUNG, light sensation.
LOCALZEICHEN, local sign (*collective* local signature).
LUST, pleasantness.
LUSTGEFÜHL, pleasure.

MASSFORMEL, formula of measurement.
MASSMETHODEN, measurement methods.
MERKLICHKEIT, noticeableness.
MESSBARKEIT, measurableness.
METHODE DER ÄEQUIVALENTE, method of equivalents.
METHODE DER EBENMERKLICHEN REIZE, method of just noticeable stimuli.
METHODE DER MINIMALÄNDERUNGEN, method of minimal changes.
METHODE DER MITTLEREN ABSTUFUNGEN, method of mean gradation.
METHODE DER MITTLEREN FEHLER, method of average error.
METHODE DER RICHTIGEN UND FALSCHEN FÄLLE, method of right and wrong cases.
METHODEN DER SINNESEMPFINDLICHKEIT, sensitivity methods.
METHODE DER ÜBERERMKLICHEN UNTERSCHIEDE, method of supraliminal differences.
MIMISCHE REFLEXE, mimetic reflexes.
MITBEWEGUNG, concomitant movement.
MITEMPFINDUNG, concomitant sensation.
MITTELBARE EMPFINDLICHKEIT, indirect *or* mediate sensitivity.
MOLL, minor.
MUSKELEMPFINDUNG, muscle sensation.
MUSKELSINN, muscle sense.
MUSKELSPANNUNGSEMPFINDUNG, sensation of muscular strain.

NACHDAUER, after-duration.
NACHEMPFINDUNG, after-sensation.
NEBENINTERVALL, overtone interval *or* interval of the second order.
NETZHAUTBILD, retinal image.
NORMALREIZ, standard stimulus.
NULLVERSUCH, blank experiment (= *Vexirversuch*).

OBERTON, overtone *or* partial tone.
OPTISCHE TÄUSCHUNG, optical illusion.
ORGANEMPFINDUNG, organic sensation.
ORIENTIRUNG, orientation.
ORTSSINN, sense of locality.

PERIPHERISCH ERREGTE EMPFINDUNG, peripherally excited sensation.
PERSÖNLICHE GLEICHUNG, personal equation.
PHANTASIE, imagination (= *Einbildung*).
PRÄCISIONSMASS, measure of precision.
PSYCHOPHYSISCHES GRUNDGESETZ, psychophysic law *or* primary psychophysical law.

RADDREHUNG, torsion *or* swivel rotation.
RAUHIGKEIT, harshness.
RAUMLAGE, position in space.
RAUMSCHÄTZUNG, spatial estimation.
RAUMSINN DER HAUT, cutaneous space sense.
REAGENT, reagent *or* reactor.
REIHENMETHODE, serial method (*affective*).
REIZ, stimulus.
REIZBARKEIT, irritability *or* susceptibility to stimulus *or* stimulation (*not* excitability).

REIZHÖHE, terminus of (effective) stimulation (*often loosely used as* = last noticeable stimulus; *cf. ebenmerklicher Reiz*).
REIZUMFANG, range of (effective) stimulation.
REIZUNG, stimulation.
REPRODUCIRBARKEIT, reproductivity (*passive*).
REPRODUCTION, reproduction.
REPRODUCTIONSFÄHIGKEIT, = *Reproducirbarkeit* (*passive*).
REPRODUCTIONSGRUNDLAGE, material of reproduction.
REPRODUCTIONSMOTIV, incentive to reproduction.
REPRODUCTIONSTENDENZ, liability of reproduction.
REPRODUCTIONSTREUE, fidelity of reproduction.
RICHTLINIE, right line.
RICHTUNGSSTRAHLEN, lines of direction.
RINDENBLINDHEIT, cortical blindness.
ROLLUNG = *Raddrehung*, *q. v.*

SÄTTIGUNG, saturation.
SCHÄTZUNGSFEHLER, error of estimation.
SCHALL, sound.
SCHMERZ, pain.
SCHWANKUNGEN DER AUFMERKSAMKEIT, oscillations *or* fluctuations of the attention.
SCHWEBUNGEN, beats.
SCHWELLE, limen.
SCHWINGUNGSZAHL, vibration rate.
SEELE (*in modern usage*), mind.
SEELENBLINDHEIT, mental *or* psychical blindness.
SEHFELD, field of vision.
SEHNENEMPFINDUNG, tendinous sensation.
SEITENWENDUNGSWINKEL, angle of lateral displacement.
SIGNALREIZ, warning stimulus, ready signal.
SINN, sense, sensibility, modality.
SINNESEMPFINDLICHKEIT, modal sensitivity.
SINNESEMPFINDUNG, sensation of special sense.
SINNESPUNKT, sensitive point *or* spot.
SINNLICHES GEFÜHL, sense-feeling *or* peripherally excited feeling.
SPANNUNGSEMPFINDUNG, sensation of strain.
SPIEGELUNG, reflexion *or* mirroring.
STATISCHER SINN, static sense.
STIMMUNG, mood.
STOSSTON, beat tone.
STREBEN, effort.
SUBJECTIVE BETONUNG, subjective accentuation.

TASTEMPFINDUNG, sensation of touch *or* tactual sensation.
TIEFENVORSTELLUNG, idea of depth *or* the third dimension.
TONHÖHE, pitch *or* tonal quality.
TONVERSCHMELZUNG, tonal fusion.
TRIEB, impulse.
TRIEBBEWEGUNG, impulsive movement.

UEBERMERKLICH, supraliminal.
UNLUST, unpleasantness.
UNLUSTGEFÜHL, unpleasant feeling.
UNTERSCHIEDSEMPFINDLICHKEIT, sensible discrimination (*better than* difference sensitivity).
UNTERSCHIEDSHYPOTHESE, difference hypothesis.
UNTERSCHIEDSSCHWELLE, difference limen *or* limen of difference.

UNWISSENTLICHES VERFAHREN, procedure without knowledge.

VERBINDUNG, connection.
VERGLEICHSREIZ, stimulus of comparison.
VERHÄLTNISSHYPOTHESE, relation hypothesis.
VERKNÜPFUNG, conjunction *or* colligation (*better than* combination).
VERSCHMELZUNG, fusion (*better than* blending).
VEXIRVERSUCH, puzzle experiment (*not so good as Nullversuch, q. v.*).
VISIRLINIE, sighting line *or* line of aim.
VÖLKERPSYCHOLOGIE, social psychology *or* psychology of nations.
VORSTELLUNG, idea.

WÄRMEPUNKT, heat point *or* spot.
WAHL, choice *or* selection.
WAHNVORSTELLUNG, delusive idea.
WAHRNEHMUNG, perception.
WAHRSCHEINLICHER FEHLER, probable error.
WETTSTREIT DER SEHFELDER, retinal rivalry.
WIDERSTANDSEMPFINDUNG, sensation of resistance.
WIEDERERKENNEN, recognize.
WILLE, will.
WILLENSHANDLUNG, voluntary action.
WILLKÜRHANDLUNG, selective action.
WISSENTLICHES VERFAHREN, procedure with knowledge.

ZEITFEHLER, time error.
ZEITHOF, time fringe.
ZEITLAGE, position in time *or* temporal position.
ZEITORDUNG, temporal arrangement *or* disposition.
ZEITSINN, time sense.
ZEITVERSCHIEBUNG, time *or* temporal displacement.
ZERSTREUTHEIT, absent-mindedness *or* distraction.
ZERSTREUUNGSBILD, dispersion *or* diffusion image.
ZUSAMMENKLANG, compound clang.
ZUSTAND, state (*not* condition; *cf. Bedingung*).
ZWANGS-, compulsory.
ZWEITHEILUNG, dichotomy.

The discussion of the terms to which an asterisk is prefixed can hardly be anything more than an expression of despair as regards the discovery of single equivalents in English.

1. *Anklingen* and *Abklingen*.—For the *farbiges Abklingen der Nachbilder*, the phrase *flight of colors* seems fairly good. For the terms themselves, *rise* and *fall* are, perhaps, the most universally applicable translations.

2. *Anlage* and *Einstellung*.—None of the words proposed by the morphologists (*proton*, *rudiment*, *primule*, *blast*, *incept*, etc.) seems to fit the psychology of *Anlage*. And *disposition* and *predisposition* are too vague to be really adequate.

3. *Anschauung* and *Warhnehmung*.—I do not know how these two terms can be distinguished.

4. *Gemüth* should have a single equivalent, but I cannot find one.

5. *Gesammtvorstellung*, *Allgemeinvorstellung*, etc.—These terms must be translated according to context. They are, for the most part, used in specific senses by different authors.

MINOR STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF WELLESLEY COLLEGE.

Communicated by MARY WHITON CALKINS.

I.—THE "CONTINUED STORY."

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With the assistance of MAUDE L. TAYLOR.

This paper briefly states the results of an attempt to discover something of the prevalence and nature of "continued stories"—imaginary and usually unwritten narratives, prolonged by their inventors, so that they go on through several weeks, months, or years. In such stories the same characters reappear in different situations and predicaments, growing often in years and in intellect. The stories themselves are cherished with a peculiar fondness, and always regarded by the authors as an especially sacred mental possession, to be shared only, if at all, with very sympathizing friends. These stories have been classified, by one of the most versatile of "continued-story-inventors," according to six main types: (1) The fairy tale and (2) the tale of martyrdom—both characteristic of early childhood; (3) the romantic and (4) the adventurous story—both belonging to late childhood and to early youth; (5) the ideal type, confined to no particular period; and (6) the practical story—characteristic of maturer years.

This paper is based on the records of 114 children in New England schools; of 214 young women, students at Wellesley College, and at the College for Women of Brown University; and of 148 men, most of them students at Harvard and in Iowa College, but a few of them in business and several of them in middle life. The following table shows the number of those who have at some time possessed a continued story:

TABLE. PREVALENCE OF CONTINUED STORY.

Age of Subjects.	ANSWERS TO QUESTION, "HAVE YOU A CONTINUED STORY?"						Sum Total.
	WOMEN AND GIRLS.			MEN AND BOYS.			
	Yes.	No and Doubtful.	Total.	Yes.	No and Doubtful.	Total.	
Adults,	100 (46.7%)	114 (53.2%)	214	20 (13.5%)	128 (86.4%)	148	352
Children,	41 (68.3%)	21 (31.7%)	62	29 (55.7%)	23 (44.2%)	52	114

From this summary it appears that nearly two-thirds of the 114 children who answered the question have continued stories, but that the proportion of affirmative answers is slightly greater among the girls. The insignificance of this difference is noticeable, and seems to suggest that the greater difference between the records of the men and the women is perhaps a result of different training. The children were members of the intermediate grammar school grades, and averaged twelve years of age. (The nature of a continued story was carefully explained to them, and all the detailed questions were asked, though the answers were not recorded). Less than a third as many men as women have continued stories, but the stories of the men are as vivid, as pronounced, and as significant as any of those recorded. One young man, for instance, reports that in his seventeenth and eighteenth years he spent six hours a day in the invention of his stories, which he characterizes as "baneful bothers, wasting time, and destroying activity even to eating and rising."

From the 93 detailed affirmative records received from adults, the following conclusions may be reached: With very few exceptions, the stories begin in childhood, and several subjects date them in their fourth year. The experience, however, is not wholly a childish one, for only 34 (of the 87 subjects who replied to this question), that is, only one-third, have lost or dropped the stories. Even a man of seventy years still carries them on. They may begin, also, in adult years. The number of stories of a given subject varies from "one," through "several," to "innumerable," or "hundreds." The length varies from weeks to years: some stories have continued since early childhood, and two-thirds of the subjects (64) have had stories with a duration of years.

With almost all the authors, comparative solitude favors the growth of the story. Many subjects, therefore, mention the hour before falling asleep as peculiarly sacred to the

"continued story," and others speak of lonely walks, of monotonous or solitary occupations like "hoeing corn," or "driving the cows." One young man says: "The story was always thought of at night, when I took long walks, often well into the morning."

The starting point in all but 12 cases is an assignable experience or a book actually read. One young woman says: "For many years I used to get the characters, station in life, place of residence, and even the remotest particulars, by telling fortunes on daisies. Then using this as a basis, I would mentally continue the stories."

In only one-fifth (18) of the stories are the characters exclusively fictitious, while in about half (47) they are entirely from real life. In three-fourths of them the author plays a very prominent part. "The heroine of everything," one subject says of herself. Often this hero is an idealized self. Thus one subject writes: "I remember in one of my stories introducing myself just as I was, and also this idealized 'I.'" Two-thirds (64) of the stories are said to "embody an ideal."

Some of the tales are recognized as distinctly helpful; others are chiefly sources of amusement. Occasionally the stories are said to be really harmful. So one student writes: "I realized that it kept me awake too long and * * * sometimes made me absent-minded, so I deliberately made myself think about other and real things * * * and gradually my interest in my story faded away." Entirely opposed to this is the experience of some one who says, "I have kept it up in order to keep my mind on one subject before going to sleep, and so as not to run over and over the events of the day."

The stories sometimes gain a great vividness, and almost the force of an illusion. So one subject says, "The story was so real that often in my dreams I was not myself, but the story character, and surrounded by the other story people." Another writes, "If I have said or done something which I wish I had not, I can, by continuing this story, make it seem as if I had never said or done it."

Most of the characters show a certain devolopment; they "grow as I grow," as one writer says. This seems to be the essential difference between the continued and the short story: the former is more intimately related to the life of its author. So one subject writes: "The long story is vitally connected with my nature." The accentuation of the emotional element in the continued stories suggests the same connection, and, on the other hand, the distinction is shown by the occasional recognition that the short stories have a greater literary value. One subject writes: "Short stories

have a greater variety of character and incident;" another says, "If I ever hit upon a fairly good plot the story ceases to be continued." Evidently the longer story follows the growth of the author's plans and purposes; embodies in concrete form his changing ideals. For this reason, one of the acutest of the observers who has answered the questions concerning her story pronounces the long story decidedly more helpful and more wholesome than the shorter ones.

It has already been suggested that the essence of the continued story, as of the more evanescent, lies in the opposing yet interacting tendencies of every individual toward self-assertion and toward imitation. The experience of the girl who says that the origin of the stories, so far as she can tell, was her "firm belief" in her "own powers," may supplement, not contradict, the testimony of the man who says that he likes "to copy a result," and that he thinks his stories "the result of a tendency toward imitation." Even the following experience, with all its self-assertion, obviously requires imitation:—

"I think the story was a continuous and progressive embodiment of my ambitions and ideals. In childhood it was popularity or skill in games, or fine possessions, in which I revelled, in imaginary conversations with other children, in which they always recognized me as superior to them, and in which I, too, had a comfortable sense of superiority. As I got older, I was more apt to picture myself as triumphant in wondrous feats of scholarship in gaining school honors. And finally I used to see myself as a teacher, and the imaginary conversations would be with the class or with the school officials. All the stories represented real ambitions which were always fulfilled, and the chief pleasure of the vision seems to have been unbounded conceit, for characters besides myself apparently existed chiefly to be witnesses of my success and to be a little envious of it."

The story which follows, illustrates admirably many of the most typical features of the "continued story:"—

"When a boy about ten years old, I read the lives of Alexander the Great and Napoleon. Then I soon began to construct these stories. I was always the hero and * * * became a great general at the head of a mighty army. I would describe my marshals and armaments, the plans of the battles and then the victorious march homeward. I always became emperor of France and conqueror of Europe, and then had a long reign filled with all kinds of interesting things. I always pictured the great funeral that followed my death and a people in mourning. I would describe in the minutest detail my children, their names, their exercises,

their studies, their marriage and the beginning of their careers. I described my home, the lakes, drives, and always my study. There was never any break. The story flowed right on, and if my attention was called away, I was always uneasy until I could begin again to weave it. I would always lie awake as long as I could, after going to bed, to work on it. I always brought my friends in and provided well for them.

"At the age of fourteen, I began to read the lives of men like Webster, Clay, Lincoln, etc. Then the story changed. My education was such as fitted me for an orator and statesman. I always became governor of my state, congressman, senator, and finally president. Every step and all my relations to friends were minutely described. I usually ended up by becoming president of a World's Congress of Peace. As soon as I had died I always started another story."

II.—Synæsthesia.[1]

By Mary Whiton Calkins.

The study of the varying forms of persisting abnormal association, usually known as "colored-hearing" and "forms," but grouped together by Theodore Flournoy, under the convenient name *Synæsthesia*, has hardly, as yet, completed the stage of scientific observation. The physiologists, with their guesses of intertwined nerve fibres, and Mr. Myers, with his prompt application of the subliminal consciousness theory, are avowedly dealing with unverified hypotheses; on the other hand, the reports of particular cases are apt to overlook the ordinary forms of the phenomenon and to disregard the frequency of the experience. For the purpose, then, of a wide yet careful survey of these phenomena of consciousness, assuming no certainty of any important theoretical outcome, it has seemed worth while to continue the statistical study of synæsthesia begun two years ago at Wellesley College. The investigation has the advantage of reaching a large number of individuals of the same sex and of about the same age, but coming from different localities and homes. The artificiality of many statistical inquiries has been avoided so far as possible, by making the questions both concrete and simple. Some of the questions of the former study[2] are here not at all considered, either because of the practical unanimity of the earlier

[1] A continuation of the Wellesley College Study of Colored-Hearing and of Forms.

[2] "A Statistical Study of Pseudo-chromesthesia and of Mental Forms." American Journal of Psychology, Vol. V, 4.

answers or because of the difficulty of gaining accurate replies.

The most general, positive conclusion of the study is the virtual demonstration of the stability of the experience. Nearly two hundred persons, questioned a second time, usually a year, always several months after the first inquiry, and without previous intimation of this verification, have been found, with only one exception, to possess the photisms or the forms at the end as at the beginning of the time, and in the same general form of the mental habit. Often the shades of color and the turns of the forms are exactly the same; in other cases, slight changes or omissions in the list of colors, or altered curves in the forms, show a close connection between colors or forms and the intervening experience of the subject, but in general type the phenomenon is an abiding one. This proof in the case of the first two canvasses of the constancy of the synæsthesia, has made it possible to omit in the last investigation the tedious process of verification, except in reference to letter-color, in which changes seem especially often to occur.

The only particular in which the results of the three canvasses seem to contradict each other is with regard to the prevalence of synæsthesia, whose per cent. of occurrence increases with each year's report. In the summary which follows, *P.* represents photisms (that is, cases of pseudo-chromesthesia); *F.* stands for forms, and includes those forms for single words, figures and objects, which Mr. Flournoy names symbols; *D.* designates not only the explicit dramatization of letters and numerals, but cases of particular fondness for especial ones; the figures under this head are given only for 1894, since in 1892 the questions were not asked, while in 1893 they were inexactly formulated.

The larger per cents. are far more likely to represent the actual frequency of synæsthesia, for the reason that the proportion increases exactly in accordance with the increasing care of the investigation. The first canvass attempted too much in trying to reach the whole college, and among the 200 who failed to respond there certainly were many who avoided the questioners in order to rid themselves of the troublesome necessity of answering. In 1893 every member of the freshman class was questioned, but the preliminary inquiry was by circular, and the traditional objection to answering statistical inquiries may be responsible for many careless, negative replies. In 1894 the class was addressed, the purpose of the investigation was explained, and the preliminary questions were answered before the students left the room. The more detailed inquiry was made either by circu-

SUMMARY I.

Frequency of Synæsthesia.

Canvass of	Subjects with[1]			Subjects with[2]			Total Subjects with Synæsthesia.	Subjects with no Synæsthesia.	Total Subjects.
	P.	F.	D.	P. & F. (& D.)	OtherCombinations.	P. or F. or D.			
'92	35 (= 6.66%)	65 (= 12.38%)		18		64	82 (= 15.61%)	443	525
'93	36 (= 16.82%)	56 (= 26.16%)		23		48	71 (= 33.17%)	143	214
'94	56 (= 23.33%)	115 (= 47.5%)	83 (= 34.58%)	38	45	62	145 (=60.41%)	95	240
Total				79	45	174	298	681	979

[1]The figures under this heading can not be counted towards the total, because the same subject may be represented in more than one column.

[2]In the figures under this head each subject is represented but once; the sum of these figures makes up the total.

lar or by personal interview. The investigation will be continued for several years, in order to obtain more material for decision, but at its present stage it seems to justify the opinion that of every ten persons five at least have some peculiar, fixed form of mental imagery, and that of these five two are likely to have photisms[1] and four to possess some mental form, while three must admit some other kind of apparently erratic association.

It is fair to add the figures of a canvass with very different results. Miss L. A. Williams questioned about 250 pupils of the Trenton, N. J., Normal School. Of these about ten per cent. were young men, and the average age of all was a little under eighteen. Only five cases (2 per cent.) of colored-hearing and six cases (2.4 per cent) of forms were found. I can explain this proportion, so much less than that of any other computation, only by reference to two facts: that 110 of the subjects "answered the questions hastily in time taken from other work;" and that some indications of the tendency "were not reported." Yet this report of a careful observer is certainly worthy of consideration.

A canvass among older people might, also, yield different results, but it should be observed that few of our subjects are conscious of any lessening of the experience. This is shown by

SUMMARY II.[2]

	INCREASE.			DECREASE.			Neither	Both	No Ans.	Total.
	Sure.	?	Total.	Sure.	?	Total.				
P.	20	8	28	14	1	15	42	0	7	92[3]
F.	38	6	44	12	4	16	90	4	17	171[3]
Total			72			31	132	4	24	263

The general character of the cases of synæsthesia appears from the following classifications:—

[1] Cf. Galton, Bleuler u. Lehmann and Flournoy for much lower estimates:—$\frac{1}{5}$ and $\frac{1}{7}$.

[2] This summany, like all those which follow, considers only the records of 1893 and 1894.

[3] 92 is the number of subjects who have photisms; 171 is the number of those who have forms; evidently the same subject may be represented in both totals.

SUMMARY III.

Varieties of Forms.

	Sure.	?	Total.
Month-forms,	141		141
Number-forms,	119		119
Day-of-week forms,	105		105
Century-forms,	48	1	49
Other forms,	95	1	96
Total forms,	508	2	510

These records, therefore, corroborate the earlier ones with regard to the order of frequency of the different forms. Month-forms lead, closely followed by number forms. Under the name "other forms" are grouped alphabet-forms; a few hour-forms; a form distinct from the number-form, which progresses from the decimals through units, tens, hundreds and the like to dexillions; a form for sharps and flats in music; two prayer forms, one for the Lord's prayer and one with a variable curve at the end, which alters with the changing character of the original petitions; and finally several symbols for places, months and numerals, with one irregular, closed curve, representing a "sudden shriek."

SUMMARY IV.

Varieties of Pseudo-chromesthesia.

With.		Sure.	?	Total.
Letters	Vowels only in 10 cases. / Consonants only in 4 cases. / Both, in 30 cases.	43	1	44
Words,		72		72
Music,		63	1	64
Numerals,		14		14
Odors,		1		1
Tastes,		6	2	8
Touches,		6	2	8
Pain,		1		1
Total forms of psuedo-chromesthesia,		206	6	212

The frequency of consonant-color still far exceeds that of Galton's and of Flournoy's subjects. The latter[1] reports 46 subjects with consonant-color to 247 with vowel-color (554 to 1,076 single cases); while this Wellesley table gives 34 subjects and 225 cases of consonant-color to 40 subjects and 134 cases of vowel-color. No table of colors is given, because not all the records have been verified. The result, however, of these records and a study of all the attempted reductions of letter-color to any rule, lead almost inevitably to the conclusion that the associations vary freely with different

[1] Flournoy, "Des Phénomènes de Synopsie," pp. 90-91.

subjects. Even Flournoy's modest "loi de clarté"[1] finds no corroboration in our records.

The noticeable frequency of i = black and o = white, in the 1892 records, fades to a mere preference for these associations over any other (9 in 26 cases of i = black or "dark," 10 in 28 cases of o = white or "pale"). On the other hand, most of the cases of music-color conform to the well-established rule: photisms for the high notes are light and those for the low notes are dark.

Detailed questions were asked, to discover, if possible, different photisms for different sounds of the same vowels, but the answers disclose a general sameness of color, with occasional changes in shade for the photisms of the long and short sounds of the same vowel. This seems to show a less common connection than is often supposed between the sound of a letter or a word and the color. This conclusion, however, is of doubtful value, for when once the letter is learned, its different sounds and shapes are almost indissolubly connected,[2] so that the color of the most important vowel-sound might conveniently stand for the letter as a whole, for every form as well as for every sound of it, even displacing previous photisms with the other sounds. The prominence of sound-color over sight-color, but the likelihood that both the sound and the appearance, which are parts of the complex letter-consciousness, are effective in the association, are shown by

SUMMARY V.

Connection of Color with Sound and with Shape.

COLOR, WHEN LETTER, WORD, MUSIC, ETC., IS

Heard (only).		Seen (only).		Both.		No Ans.	Total.
28	9	3	3	56	5	5	92

The figures in the right hand columns indicate cases which are not counted in the totals, because represented in still another column. This occurs when different photisms of the same subject are occasioned by different stimuli.

The remaining results of the study of pseudo-chromesthesia are grouped together, with reference to their bearing on the attempted explanation of the experience. From these records

[1] *Op. cit.*, p. 66.
[2] Cf. Flournoy, *op. cit.*, pp. 48-49.

it is clear that some, at least, of these photisms must be explained as due to natural associations. The instances in which these explanations are definitely given are summarized below.

SUMMARY VI.

Explanation of Pseudo-chromesthesia.

Nature of Pseudo-chromesthesia.	THE EXPLANATION IS						Total Explanations.	No Explanations.	No Ans.	Total Cases.
	THROUGH ACT. OCCURRENCE.		EMOT'L.		BOTH.					
	Full.	Partial	Full.	Partial.	Full.	Partial.				
Letter-color,	2	3	0	0	0	0	5	28	11	44
Word-color,	12	12	4		8	1	37	26	9	72
Music-color,	5	1	15	9	2	3	3	19	10	64
Color with Numerals,	0		0		0		0	0	14	14
Color with Touches, Tastes, Odors, Pain,	0		0		0		0	0	18	18
Total,	19	16	19	9	10	4	77	73	62	212

It should be added that among 40 cases of the occurrence of both letter and word-color, there are 13 in which the word-color is known to have been earlier in occurrence than the letter-color, while in only three cases the letter-color is remembered as earlier. From the summary it already appears that word-color is very[1] susceptible of explanation through some actual experience, and, in fact, 10 of these 13 cases are wholly or partly explained by the subjects. It is possible, therefore, that these instances of letter-color are due to forgotten connection with natural word-associations.[1]

The prominence of association through emotional experience is marked, especially in regard to music-color. This agrees with the results of the earlier canvass[2] and seems to indicate that here, as in the case of so many psychological problems, the ultimate solution may be in unanalyzable terms of feeling. "Gay disposition, gay color," says one subject; "if I admire name or character, it is through liking for color." "It's

[1] Cf. *op. cit.* AMERICAN JOURNAL OF PSYCHOLOGY, V, 4, p. 447.
[2] Cf. *op. cit.*, AMERICAN JOURNAL OF PSYCHOLOGY, V, 4, p. 446.

the feeling I get from my music," another says; "I always imagine those colors with those emotions." "Things which make me happy," writes a third, "are always light pinks, blues and yellows, while sad things are always dark."

Many of these explanations are, of course, fragmentary and incomplete, and it is possible that some are mere instances of paramnesia or explanations after the event. It is just as likely, however, that many instances of the origin through ordinary association have been forgotten. Certainly the existence of any such natural explanations diminishes the necessity and the probability of the theory of physiological abnormality. Cases in which the experience is definitely useful or pleasant also favor the natural theory, and these are not inconsiderable in number.

SUMMARY VII.

Utility and Pleasurableness of Pseudo-chromesthesia.

	Yes.		No.		Neither.	Both.	No Ans.	Total.
	Sure.	?	Sure.	?				
The subject's memory is helped,	12	4	50	7			19	92
The subject finds pleasure in "colored-hearing,"	44	7	3	1	24	5	8	92

There are few cases of assisted memory, but some of these are very marked, as, for instance, that of the student who says, "If I hear an opera, I can come home and almost play it by colors; I know what chords make a given combination of colors." The pleasurableness of the experience is very common, and might be a reason for the perpetuation of a color and sound association accidentally formed.

For the physiological theory the strongest argument is the undoubted hereditary tendency of synæsthesia. The answers to the questions bearing on this point are not summarized, since the subjects, who very likely have never heard of any colored-hearing or forms among members of their families, are so likely to reply by a rash "no" or by a misleading "doubtful." The frequent repetition of the experience within a family and its continuance from one generation to another are acknowledged, however, by all observers, and certainly suggest the existence of cerebral peculiarities. But these may be the result or the accompaniment, not the cause, of the synæsthesia, which may still be referred to use-

ful or pleasant associations. Cases in which the pseudo-chromesthesia rises to the stage of hallucination are also such as lend themselves readily to a cerebral explanation. These are presented in the next table, but it should be observed that the statistical method, even when supplemented, as in this case, by simple experiments, is peculiarly unfitted for an investigation of hallucination, since the questions themselves may suggest a false memory of hallucination; even so, the affirmative answers to these questions are few. They include cases in which a page seems to the reader actually tinged with the shade of the "colored" letter or word, as when one subject says, "The paper grows orange-pink as I look at *a* on a page;" and instances of after-images, like that of the student who answers, "Sometimes when I look up very quickly, I have the same color [as that of letters or word], when I don't want to have it at all."

It might also be urged that instances in which the color is in very distinct form and is very definitely located are more likely than the cases of shadowy and vague color to involve peculiar cerebral accompaniment. This conclusion is of questionable validity, but the figures bearing on these considerations are added and show that the color is usually in indistinct form, but almost always definitely located—generally, it may be added, in front of the subject.

SUMMARY VIII.

Form and Location of Color. *Hallucination.*

	Definite.	Indefinite.	No Ans.	Total.	Yes.			No.			No Ans.	Total.
					Sure.	?	Total	Sure.	?	Total		
Form,	31	47	14	92	13	8	21	54	6	60	11	92
Location,	64	15	13	92								

In conclusion, therefore, it may be said that our results do not demonstrate either theory of pseudo-chromesthesia to the exclusion of the other, but that they favor the "psychological" explanation through natural association, by proving the existence of some cases, at least, which demand this explanation.

Among the 171 persons who have forms, we find only 4 who are certain of any hallucination and 7 who answer by a doubtful affirmative. One of the rare cases in which the form is a positive hindrance seems to approach in its

vividness the plane of illusion: of a number form, which is a spiral prolonged to infinity, the possessor writes, "[my form] makes mathematics harder, for, *e. g.*, in algebra, when I must substitute ∞ throughout an equation, I get so lost in the ∞ that I can't get at it at all."

The explanations and the cases of usefulness are, however, very frequent; the shapes are in most cases familiar, usually circles, rectangles or lines; the turns of the number-forms in the great majority of cases are at the most prominent numerals, the 5's, the 10's and the 12's.[1] So it is in the highest degree probable that most of these forms originate in the self-helping, topographical imagination of children introduced to the intricacies of number and word series. The frequency of slight variations in the forms from year to year—bends to right instead of to left, or upward rather than downward—favors the theory of natural association by showing, as has been said, a connection with the adult, as well as with the childhood experience, a certain sensitiveness to changes in the methods of thought and of life. The physiological theory can hardly account, in so simple a manner, for these unimportant yet definite alterations.

SUMMARY IX.

Explanation and Utility of Forms.

Forms have	YES.			NO.			No Ans.	Total.
	Sure.	?	Total Affirmative.	No.	Hindrance	Total Negative.		
Explanation,	98	31	129 (25.29%)	318		318 (=62.35%)	63	510
Utility,	182	27	209 (40.98%)	243	6	249 (=48.82%)	52	510
Explanation, or Utility, or Both.			278 (54.50%)			220 (43.13%)	12	510

The proportion of definite explanations is thus considerably greater than that of the earlier study,[2] and in view of the great probability of forgotten origins, the natural theory of forms seems overwhelmingly probable. "The nature of the month-form," one student writes, "is probably due to the

[1] Cf. *op. cit.*, AMERICAN JOURNAL OF PSYCHOLOGY, V, 4, p. 449.
[2] *Op. cit.*, AMERICAN JOURNAL OF PSYCHOLOGY, V, 4, p. 448.

nature of my school work. From January to June there is a gradual letting up of the strain; July and August are the calm months, and with September begins the up-hill work." "My number-form," says another, "I can trace back to a game which I played in childhood. The figures were in small blocks." A third explanation refers the origin of the form to kindergarten days: "My number-form originates, I think, in those frames with colored beads strung upon wires, by means of which children are taught to count, add, subtract, etc."

The testimonies to the utility of the forms are no less explicit. These which follow are representative: "The only way I can remember dates is that other dates are proportional to them (on a form of concentric circles). I remember 1,625 because it is at an angle of 30° from the present." "I am almost entirely dependent on my form for remembering dates, appointments, people and places; and * * * I think it would be impossible for me to add the smallest numbers without the aid of my number form." "My alphabet-form helps very much in type-setting." "When I said I would come here, I 'put it down' on my form."

The significant number of those who believe that they are helped by their forms to memorize facts, to remember dates and to perform mathematical operations, emphasizes the wisdom of such educational use of forms as has already been made by Miss Adelia Hornbrook.[1] Indeed, the use of charts and of diagrams is in itself a suggestion of mental forms, for calendars and primer pages lie at the basis of many month, week and alphabet forms. To make these suggestions more definite, and, in particular, to impress the child's memory, as Miss Hornbrook does, with some simple number-form, seems a reasonable, pedagogical application of these forms. Such aid to the visual imagination might not aid the essentially "ear-minded" children, but it could do no harm unless unduly pressed.

The elaborate dramatization of letters, numerals and musical notes,[2] by which they are endowed with physical and with psychical characteristics, so that they often become actors in entire little dramas among themselves,—this complex experience may probably be referred to the commoner and simpler phenomenon of especial like or dislike for certain letters or numbers. All these cases may be classified as follows:—

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[1] *Educational Review*, V, p. 467.
[2] *Op. cit.*, AMERICAN JOURNAL OF PSYCHOLOGY, V, 4, p. 454.

SUMMARY X.

Personification.

Forms of Personification.	Yes.	No.	Total.
Like or Dislike,	75	8	83
Dramatization,	46	37	83

Examining more closely these instances of like and dislike, we find that there are fifty per cent. more such associations with the numerals than with the letters, which indicates that the numerals, as objects of more intense intellectual effort, are more likely to become factors of emotional association. Still more carefully observed, these numerical associations disclose the existence of a marked preference for the even numbers.

SUMMARY XI.

Even and Odd Numbers.

Numbers.	EVEN NUMBERS.			ODD NUMBERS.			Indefinite Ans.	Letters only Liked, etc.	Total Liked and Disliked.
	Even Only.	Even with Odd.	Total.	Odd Only.	Odd with Even.	Total.			
Liked,	22	12	34	12		12	21	8	75
Disliked,	4		4	38	3	41	14	16	75

There seems to be a special fondness for 2, and—among the odd numbers—for 5, but a common aversion for prime numbers like 7, 11 and 13. One would almost certainly infer that these feelings have their root in the actual experience of facility in the use of even numbers, and of difficulties with the unyielding indivisibility of prime numbers, and the explicit testimony of one-fourth of our subjects confirms this view. In these cases of personification, therefore, as well as in the other forms of synæsthesia, the "psychological" theory seems the simpler and the more probable.

QUESTIONS ON SYNÆSTHESIA.

These questions are based upon a list formulated after the careful study of more than 80 records of synæsthesia. They have been re-cast after the experience gained by using them during two years,

for 200 subjects, and after the addition of questions suggested by Flournoy's and by Gruber's classification.

Questions which seem to the writer of secondary importance in the theoretical consideration of the subject, though necessary to a complete description, are starred.

It is suggested that the first step in a systematic investigation of synæsthesia should be to ask the preliminary questions which demand simple "Yes" and "No" answers. When the replies have been sifted, the more detailed questions may be sent to all who have answered affirmatively. Canvasses of men's colleges, or college classes, of associations of people in middle life, of schools of children and young people, and of the accidentally blind and deaf, would yield especially valuable results. Materials and more detailed suggestions for such inquiries will be supplied, and the results gladly received by

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SYNÆSTHESIA.

PRELIMINARY QUESTIONS.

Answer by "Yes," "Yes?", or "No." Do not fail to answer "Yes?" *not* "No," if in any doubt.

I. Do you think of particular colors in connection with letters of the alphabet, or numerals, or proper names, or musical sounds, or in any other unusual connection?

II. Do you think of numerals, or names of months, days or years, or of any series of words, as arranged in particular shapes, like circles, squares, zig-zags, or very long lines?

III. Do single numerals, letters, musical notes, etc., make you think of different shapes?

IV. *a.* Do you especially like or dislike any numerals, letters, etc.?

b. Do numerals, letters, etc., seem to you to be like people?

DETAILED QUESTIONS.

Note.—Many of these questions may be answered by "Yes" or "No," but fuller replies are preferable. It is hoped that all questions will be answered, but the less important ones are starred.

A. Pseudo-chromesthesia.

I. Do you habitually or frequently "seem to see" colors or variations of light and shade, in connection with certain letters, words, objects, sounds, or other sensations? If so, mention the colors connected,

a. With letters.

1. With vowels.

ā (as in fāte), ă (făt), ä (fär).
ē (" mē), ĕ (mĕt).
ī (" mine), ĭ (pĭn).
ō (" mōte), ŏ (nŏt).
o͞o (" mo͞on).
ū (" mūte), ŭ (tŭb).
ȳ (" tȳpe), y̆ (sy̆mbol).

2. With diphthongs.
æ au ay ei ew œ ou
ai aw eu ey oi ow.
3. With consonants.
b d
c f
etc. (Mention all cases.)

b. With words.
1. Names of people. (Mention instances.)
2. Names of places. (Mention instances.)
3. *a.* Names of months. (Mention all cases.)
b. Names of days of week. (Mention all cases.)
4. Common terms.
a. With all words, or with a few?
b. With any particular parts of speech?
c. With abstract terms?

c. With sounds.
1. With musical sounds.
a. Different pitches.
(1) High.
(2) Low.
b. Different intensities.
(1) Loud.
(2) Soft.
c. Different keys. (Mention all cases.)
d. Different instruments, *e. g.*, violin, piano. (Mention all cases.)
e. Different composers, *e. g.*, Chopin, Handel.
f. Different pieces of music.
2. With noises. (Mention all cases.)

d. With numerals, *e. g.*, 1, 3, 9. (Mention all cases.)
e. With pictures or objects, which are
1. Still.
2. In motion.
f. With tastes. (Mention all cases.)
g. With odors. (Mention all cases.)
h. With skin sensations. (Mention all cases.)
1. Contact.
2. Pressure.
3. Temperature.
4. Sensations of movements.
k. With pains. (Mention all cases.)

Answer questions II and III with reference to each sort of color: that for letters, words, music, numerals, tastes, etc.

*II. a. Does the color appear
1. *Only* when the letter (word, music, etc.) is *heard?*
2. *Only* when the letter (word, music, etc.) is *seen?*
3. *Both* when the letter, etc., is *heard* and *seen?*

Note.—Which seems to have been earlier, color with sound or with form?

b. Does the color appear
1. When the letter, etc., is imagined as *heard?*
2. When the letter, etc., is imagined as *seen?*
3. In both cases?

c. Does the color appear invariably or occasionally?

*III. What is the location of the color?

a. Is it in tridimensional space, *e. g.*, in front of you, to the right, etc.? or,

b. Is it as if on a page? or
c. Is it impossible to give the location?

*IV. What is the *shape* or *form*
a. Of color with music and noises?
b. Of color with tastes, etc.?
c. Of color with letters and numerals:—
1. Does each letter and numeral appear as if printed or written in colored ink? or,
2. Has the color some other definite shape? or,
3. Has the color a vague and indefinite shape?
d. Of color with words:—
1. Is each letter colored separately?
2. Are all letters colored, but of one color?
3. Is the word printed or written, in a neutral tint on a colored background?
4. Has the color some other definite shape?
5. Has the color a vague and indefinite shape?

V. (If you have word color)
a. Give your color for
1. Sara.
2. Lottie.
3. Date.
4. Harry.
5. Samuel.
6. Fate.
7. Door.
8. *a*. Meet.
b. Meat.
9. *a*. Carrie.
b. Carry.
10. Alice.
11. Edith.
12. Oscar.
13. Anna.
14. Stephen.
15. Clifford.

b. How does the color of a word seem to you to be determined?
1. Does it follow the color
a. Of the initial letter?
b. Of a repeated letter?
c. Of a vowel or of vowels?
d. Of a consonant or of consonants?
e. Of an accented vowel or consonant?
2. Does each letter have its own color as when perceived alone?
3. Is the color a mixture of the colors of the different letters?
4. Does the color follow the prevailing sounds, so as to be the same for rhymed words?
c. Was word-color earlier than letter-color?

Answer question VI with the fullest details possible.

VI. a. Have you any explanations of your colors, by association, *e. g.*,
1. Of the *letters*, with the colors of blocks or pictures from which they were learned?
2. Of the *numerals*, with some similar objects?
3. Of *names of people*, with the color of hair, or of eyes, or of garments of particular people?
4. Of *names of places*, (*a*) With colors of a map?
(*b*) With varying colors of foliage, etc.?
5. Of names of months with season-colors?
6. Of favorite letters or words with favorite colors?
Note.—Name your favorite colors.
7. Of musical tones with emotions, and so with colors producing the same emotions?
8. Of musical selections with the colors of real or imagined scenes?

b. Mention any other explanations for the origin or for the alteration of your colors.

VII. a. Do your "colored" words aid your memory in spelling?
b. Do your "colored" notes aid your memory for music?
c. Are your colors of any other assistance? (Give full details.)

VIII. Is your pseudo-chromesthesia a source of
a. Pleasure? or
b. Pain? or
c. Neither? or
d. Partly of pleasure, partly of pain?

IX. Have any of your immediate family or other relatives
a. Pseudo-chromesthesia (colored hearing)?
b. Forms?
c. Any similar habit?

*X. Did your pseudo-chromesthesia begin
a. In childhood? or
b. Later? or
c. Part at one time, part at another? (Give details.)

*XI. Has your pseudo-chromesthesia
a. Increased? or
b. Decreased? or
c. Neither? or
d. Part increased and part decreased? (Give details.)

XII. a. Is your pseudo-chromesthesia so strong that
1. If you hear, read, or imagine one of your "colored letters," words, etc., while looking at a white background, the white becomes colored?
2. You have an after-image of the color?
3. A page on which your "colored" words, etc., are printed seems flecked with the color?
4. The "color" of a name actually seems to intensify or to change the color of the dress of the wearer?

b. Mention any other cases in which your pseudo-chromesthesia ever becomes or tends to become hallucination.

XIII. Have you any such peculiar associations
a. With sounds? *e. g.*, do colors suggest musical notes?
b. With tastes, odors, etc.? *e. g.*, do musical notes suggest tastes, odors, etc.?

*XIV. Mention any personal details which bear on the subject, *e. g.*,
a. Are you, in any sense, an artist?
b. Are you, in any sense, a musician?

B. Forms.

I. a. Draw your forms for *series* of words, numerals, etc.
1. For numerals.
2. For months.
3. For days of the week.
4. For years or centuries.
5. For the alphabet.
6. For any other forms.

} Indicate the position on each form of numerals, names of months, letters, etc.

b. Draw your symbols, if you have any, *i. e.*, forms for single numerals, letters, etc.
1. For numerals, *e. g.*, 1=△ 2=○ 3=+ 4=ξ
2. For letters.
3. For musical notes.
4. For words.

*II. Please state with reference to each form whether

a. The numerals, names of months, etc., appear as if printed (or written) on the form.
b. The form is colored.
c. Any images of scenes or of objects appear in the forms.

*III. Please state with reference to each form and symbol, whether
a. The form, etc., is flat, like a plane surface.
b. The form, etc., is flat, as if printed on a page.
c. The form, etc., extends in three dimensions.

*IV. Please state with reference to each form and symbol, whether
a. 1. The form, etc., is in front or back of you.
2. The form, etc., is above or below you.
3. The form, etc., is right or left of you.
b. You always imagine yourself in relation to the form, etc.

*V. a. Do the forms appear invariably or occasionally?
b. Do the forms appear as wholes or in sections?

VI. Have you any explanations of the origin or of the particular shape of your
a. 1. Number-form?
2. Month-form?
3. Day-of-week-form?
4. Year or century-form?
5. Alphabet-form?
6. Other forms?
b. Symbols (single forms for numerals, etc.).
Can you refer any forms or symbols to lessons or games of childhood? Are they like any pattern of wall paper or carpet? like a calendar? like a clock face? etc.

VII. a. Does your number-form help you
1. In remembering dates?
2. In any mathematical operation?
3. In any other way?
b. 1. Does your month-form
2. Does your day-of-week-form
3. Does your year or century-form
} help you in remembering dates or appointments?
c. Does your alphabet-form help you
1. In spelling?
2. In looking up words in a lexicon?
3. In any other way?
d. Does any other form help you?
e. Do your symbols help you?

*VIII. Does the possession of the forms and symbols give you
a. Pleasure?
b. Pain?
c. Neither?
d. Partly pleasure and partly pain?

IX. Have any of your immediate family and other relatives
a. Pseudo-chromesthesia (colored-hearing)?
b. Forms or symbols?
c. Any similar mental habit?

*X. Did your forms and symbols begin
a. In childhood?
b. Later?
c. Part in childhood, part later?

*XI. Have your forms and symbols
a. Increased?
b. Decreased?
c. Neither?
d. Part increased and part decreased?

XII. a. Do you ever feel as if the forms and symbols had an actual, external existence?
b. Do they ever involve you in any other sort of hallucination?
*XIII. Mention any personal details which bear on the subject, *e. g.*,
a. Are you an artist?
b. Are you an architect?
c. Are you especially fond of system and method?

C. Personification.

I. Like and dislike
a. For numerals.
1. Do you especially like any numerals?
2. Do you especially dislike any numerals?
Give reasons in both cases.
b. For letters.
1. Do you especially like any letters?
2. Do you especially dislike any letters?
Give reasons in both cases.
c. Mention and explain any similar cases of like and dislike.
II. Dramatization.
a. With numerals.
1. Do numerals seem to you to have physical characteristics? (*e. g.*, is 1 short and fat, 4 tall and thin, 7 brunette?)
2. Do numerals seem to you to have mental and moral characteristics? (*e. g.*, is 8 upright, 14 mean, 16 kind?)
Give reasons for all cases.
3. Is the dramatization so complete that the numerals seem like persons? (Give details.)
b. 1. Do letters seem to you to have physical characteristics?
2. Do letters seem to you to have mental and moral characteristics?
Give reasons for all cases.
3. Is the dramatization so complete that the letters seem like persons? (Give details.)
c. Mention and explain any similar cases of dramatization.
D. Mention any other cases of peculiar association.

PSYCHOLOGICAL LITERATURE.

I. SOME RECENT STUDIES OF PAIN.

1. *Pain, Pleasure and Æsthetics.* MARSHALL. London, 1894.
2. *Beiträge zur Physiologie des Schmerzsinns.* PROF. DR. M. VON FREY, Ber. d. math.-phys. Klasse. d. k. Sächs. Ges. d. Wiss. July and December, 1894.
3. *Die Sensibilität der Conjunctiva und Cornea des menschlichen Auges.* DR. W. A. NAGEL. Pflüger's Archiv, Bd. LIX, 1895, pp. 563-595.
4. *The Origin of Pleasure and Pain.* DR. H. NICHOLS. Phil. Rev., I, pp. 403-432, 518-534.
5. *Schmerz und Temperaturempfindung.* PROF. DR. Z. OPPENHEIMER, Berlin. Georg Reimer, 1893.
6. *Ueber den Schmerz.* PROF. DR. A. GOLDSCHEIDER. Berlin, 1894.

Says Goldscheider, in his recent work, "Ueber den Schmerz," "It is a shame that we possess such insufficient knowledge concerning the character of pain—those symptoms which represent the essential part of all bodily suffering of man." This statement is true if we have in mind our knowledge of the origin and mediation of pain; *i. e.*, whether it is of physiological or of psychical origin; whether it is mediated by specific nerves, or by any or all sensory nerves; whether pain is to be considered as a *quale* of a sensation, a separate, coördinate sensation, or only an intense degree of a sensation;—for on all these points our knowledge of pain is far from satisfactory. On the other hand, if we have in mind the many facts we know concerning pain, its influence on our mental and bodily states, its importance as a guide to health or disease, as well as its value in leading to higher moral and religious action, there is, perhaps, no sensation about which we know more. The difficulty in finding a theory for pain is due, perhaps, in large part, to the great number of isolated facts we already know concerning it. If we knew less we might be willing to accept any one of the many theories already proposed, but as it now is, no theory seems to answer all. The primordial character of pain makes it one of the most difficult of experiences to analyze. It comes to us after untold ages of accumulated experiences, mixed with other, later-developing sensations, from which it is next to impossible to separate it.

Some of the Known Facts.

What are some of the isolated facts that we know concerning pain? To enumerate, without endeavoring to follow any particular order, we may state:

That a stimulation of pressure (touch) is perceived sooner and fades away quicker than a painful stimulus occurring at the same time and in the same place—that is, under normal conditions, pain and touch excitations occurring together are separated in time before reaching consciousness. Again, under the influence of anæsthetics, pain and not touch may be temporarily destroyed, or *vice versa;* for instance, cocaine and chloroform destroy pain and not touch, while saponin destroys touch and not pain. Under clinical and pathological conditions, such as traumatism, syringomyelia, etc., pain may be destroyed without any other sensation being materially injured. Sectioning the gray matter of the spinal cord destroys the perception of pain from parts below the section, without destroying the sense of pressure (touch).[1] In multiple neuritis and certain lesions of the cord, the sense of touch may be destroyed without that of pain. Pain may be produced by an electric spark, or by thermal stimuli, without any sensation of touch.

Again, as shown by Naunyn and others, pain is the result of a summation process. Naunyn[2] found, in cases of tabes dorsalis, that a mechanical stimulus (produced by touching the skin of the foot with a hair, etc.), which is below the conscious threshold of either pain or touch, if repeated a great many times per second (60 to 600), will be perceived, after a few seconds (6 to 20), as a pain which soon becomes unbearable. The same effect of summation can be shown with electric stimulation, where, with a weak current, a few shocks per second are not painful, but a large number per second are decidedly so.

Notwithstanding the above cases, in which it seems possible to separate pressure (touch) and pain, yet normally, especially under mechanical stimulation, pain is seldom found without being preceded or accompanied by touch. It is probably the mixing of these different cutaneous sensations with pain that enables us to locate the latter, and gives to it the peculiar coloring which we designate as acute, sharp, smarting, stinging, boring, lacerating, lancinating, griping, gnawing, aching, bearing-down, etc., according as the painful feeling is mixed more with one or another of the cutaneous sensations. Owing to the insufficiency of language to express such states of consciousness, it is difficult to make the above terms more simple, or even to be sure that the same term is used for the same thing by different individuals. A pain may vary, of course, through any or all of these different qualities at different times.

Pain, according to Erb,[3] is not a sensation of a peculiar form, but one of higher degree; therefore, every sensory stimulus is capable of producing pain, if it reaches sufficient intensity. But we can hardly retain this statement among those of generally accepted facts, for the statement is questioned by those who hold that the higher senses (sight, hearing, etc.), as such, do not give physical pain. On the other hand, every kind of stimulation, mechanical, thermal, electrical and chemical, may excite pain. Any disturbance of nutrition or of circulation may also produce the same result.

Pain is generally accompanied or followed by inflammation of the parts, but what particular relation this bears to pain is not known. Most would agree that, in many cases at least, the pain was due to the inflammation; but Radcliff[4] and some others take the ground

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[1] Schiff, "Lehrbuch der Muskel- und Nerven-Physiologie," S. 251 ff.
[2] Arch. f. experim. Pathol. u. Pharmak. Bd. XXV., S. 272 ff.
[3] Krankheiten der peripherischen cerebro-spinalen Nerven, 1874.
[4] Radcliff, Lectures on Epilepsy, Pain Paralysis, etc., London, 1864. John Churchill.

"that pain of a neuralgic character is antagonized rather than favored by inflammatory excitement of the nervous system." And, further, "where pain seems to be associated with active fever and inflammation, it has been seen that the place of the pain is in the cold stage, before the establishment of the hot stage of the disorder, and not in the hot stage itself—in the stage of irritation preliminary to the inflammation, and not in the stage of actual inflammation." As is well known, pain is a very important sign of disease, and becomes of great value to the physician in his diagnosis, but the pain is very often located at a distance from the seat of the disturbance. For instance, disease of the hip-joint causes pain in the knee; inflammation of the liver, or in the diaphragm, causes pain in the right shoulder; valvular lesion of the heart may produce pain in the left arm; irritation of the stomach may cause pain in the head; stone in the bladder may produce pain in the outlet of the urinary passage; spinal lesions are nearly always referred to some more peripheral part.

Individuals, as well as nations, differ greatly in their sensitiveness to pain. The Irishman is more sensitive than the Scotchman; the Latin race is more sensitive than the Teutonic. There are age, class and sex differences, though the data here are very limited (Lombroso, and later MacDonald[1]). It is also true that the same individual differs considerably from day to day in his sensitiveness to pain without any apparent cause.

Again, a mechanical (pressure) stimulation, which is not at all painful at first, will, if long continued, become not only unpleasant and annoying, but actually painful. Here continuation of pressure seems to change a touch sensation into a painful one. There are some parts of the body, *e. g.*, the cuticle, nails, hair, ligaments, etc., that are never painful; while there are other organs, *e. g.*, the lungs, liver, kidneys, intestines, mucous membrane, tendons, etc., which, under normal conditions, function painlessly, but under pathological conditions become the seat of very severe pain.[2]

To the above may be added the pains due to general depression, fatigue, hysteria, hypnotism, etc. By hypnotic suggestion the body may, on the other hand, be rendered insensitive to pain, while all other sensation remains intact (Witmer[3]).

Still another interesting fact has been brought out by Nagel, namely, that a current of hot air striking the conjunctiva, cornea or mucous membrane of the tongue, is never perceived as warm, but as cold, unless too hot, in which case it feels cold and painful. The other parts of the body, however, perceive the warm current as warm.

Some Recent Theories of Pain.

In order that any theory of pain may be accepted as final, it must explain all the above facts as well as many others not mentioned. There are almost as many pleasure-pain theories as there have been writers upon the subject, and the subject has by no means suffered for want of writers. Each theory may be satisfactory to the man who proposed it, but few can be said to be satisfactory to large numbers, while none seem to be satisfactory to all.

I wish to confine myself in this article to a few of the recent theories of pain, which cover pretty nearly the present status of the subject. These theories may be divided into three groups:

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[1] AM. JOURN. PSYCH., Vol. VI., p. 621.
[2] Oppenheimer, "Schmerz und Temperaturempfindung."
[3] *The Journal of Nervous and Mental Disease*, April, 1894, p. 219, note.

1. Those which represent pain as a *quale* of sensation.
2. Those which class pain as a distinct sensation.
3. Those which class pain simply as a certain degree of sensation.

Under the *quale* theory may be classed nearly all the ancient writers on the subject as well as many of the present day. By far the best exposition of this theory is given by Mr. Marshall in his recent book, "Pleasure, Pain and Æsthetics." This most excellent treatment of the subject has given a new impetus to the *quale* theory as well as to the whole study of emotion. A careful reading of the book makes one feel that there is but little left to be said on that side, and one must admit the great importance of the work, even though he may not be able to bring his mind to harmony with the author's view.

Marshall gives three hypotheses to account for pain and pleasure, of which he accepts the last: (1) "That pleasure-pain modes are the fundamental elements from which all mental life develops, a hypothesis which, apart from other oppositions, is negatived by the fact that our mental life is not developed on two distinct lines, viz., of pleasure and of pain." (2) "That in pleasure-pain we have a special mode of mental activity, a series *sui generis*, unlike and standing apart from any other mental state in character and means of genesis, which, however, is connected with all other mentality in some subtle way." (3) "That pleasure and pain may be differential qualities of all mental states of such nature that one of them must and either of them may, under their proper conditions, belong to any element of consciousness." "Under these hypotheses pleasure and pain are primitive *qualia*, which may appear with any mental element; simple, primitive ideas in the Lockian sense, and, therefore, correctly classed by him; simple primary differentiations of presentation, which are grasped by us essentially after the same manner in which we know the mind to act in other directions, but in the most primal forms of such action." Mr. Marshall says further: "That psychic life is not divided on the lines of pleasure, is no objection to a view which makes pleasure and pain *qualia* of all presentation composing our psychic life as we know it, for the distinctly marked-off psychic states are not supposed to be developments from the pleasure-pain modes, but states still subject to these qualifications.

As to the physical basis of pain, he holds that "no special nerve organs and no distinct differentiations of such organs are to be looked for to account for the *qualia* which relate to the whole field of mental life, for their physical conditions, whatever they be, must be looked for in all that which we learn to look upon as the physical basis; *i. e.*, in all of nerve necessary for mentality whatever special parts are, for any one moment, called into activity. Each case of distinct presentation may thus be said to bring forward its own pleasure or pain organ, as it were, fitted to act under proper conditions." The old Aristotelian idea that "the activity of the organ of any content, if efficient, is pleasurable, if inefficient, is painful," is discussed and changed to the following principle: "All pleasure-pain phenomena are determined by the action in the organs concomitant of the conscious state, as related to the nutritive condition of the organs at the time of the action."

In particular his hypothesis is: (1) that "pleasure is experienced whenever the physical activity, coincident with the psychic state to which the pleasure is attached, involves the use of surplus stored force—the resolution of surplus potential into actual energy, or, in other words, whenever energy involved in the reaction to a stimulus is greater in amount than the energy which the stimulus habit-

ually calls forth;" and (2) that "pain is experienced whenever the physical action which determines the content is so related to the supply of nutriment to its organs that the energy involved in the reaction to the stimulus is less in amount than the energy which the stimulus habitually calls forth." Or, in other words, that "pleasure and pain are primitive qualities of psychic states, which are determined by the relation between activity and capacity in the organs, the activities of which are concomitants of the psychoses involved."

Without quoting farther, we may sum up Mr. Marshall's positions as follows: (1) That pleasure and pain are general qualities, one of which must, either of which may, belong to any fixed element of consciousness. (2) That emotions are the psychic coincidents of relatively-fixed, coördinated, instinctive activities arising upon the appearance of definite objects, and, therefore, only indirectly connected with pleasure and pain. (3) That æsthetics should be treated as a branch of hedonics, or the science of pleasure. (4) That pleasure and pain are determined by the efficiency and inefficiency, respectively, of the organs active in coincidence with the pleasurable or painful mental elements; that efficiency and inefficiency are functions of the relation between activity and nutrition, pleasure being dependent upon the use of surplus stored force, and pain upon conditions under which the outcome of the organ's activity is less than should be expected in consideration of the energy involved in the stimulus.

It seems more proper to designate Mr. Marshall's classification of pain and pleasure as a psycho-physical than a psychological one, and yet his pleasure-pain theory is distinctly psychological, making pain and pleasure due to psychic states as wholes, rather than to the disturbance of any particular organ. Under his treatment, his theory becomes exceedingly flexible, and seems capable of answering nearly all the known facts. He says, in reference to those pains which often seem separate or distinct in themselves, that they do not invalidate his theory, for, under extreme conditions of excess of activity as related to nourishment, the psychosis of relation should be vivid. Pains from organs which are normally not painful, but which become so under hypernormal conditions, are due to the fact that these organs are normally so regular in their rhythm that they are not called upon to act powerfully, and, therefore, have little capacity or use for surplus stored force, consequently any hypernormal condition would cause them to act painfully. The same reason may account for some organs being incapable of pleasurable stimulation.

As to the secondary sensation, occurring in case of a prick, he assumes a second set of nerves, which are brought into action after touch, and respond painfully on account of their little storage capacity. Analgesia, he says, may be answered by one sense being obliterated, while the other is not cut off. But it is difficult to understand how either of these statements can be made to coincide with his theory. In the first statement he has separated the pleasure-pain *quale* from touch, and has made it include the whole of the secondary sensation. In reference to the second statement, if the *quale* may be separated from the sensation, either by different paths of conduction or by disease, there would then be a *quale* without a sensation, or a sensation without a *quale*; either of which would seem fatal to the *quale* theory. It has been shown quite conclusively, however, by Schiff and others, that pain has a different path of conduction in the spinal column from most of the other haptic sensations. To say that under certain conditions the *quale* may

become so intense as to blot out the rest of the sensation, is not satisfactory, for what reason have we to say the rest of the sensation is there, only obscured from consciousness? Furthermore, pain not only occurs frequently unaccompanied by other sensory elements, but may be located at a distance from the seat of the disturbance, and also seems to have a quality of its own. Again, it may be asked, should not the quality of a sensation become more noticeable with practice (exercise), *i. e.*, the quality of wine by experience in tasting—the perfume of flowers by experience in scenting—tints or shades of color of a visual sensation by practice in seeing? Exercise, however, seems to increase instead of decreasing the pain threshold.

According to the *quale* theory, it would not seem possible that the injury of any organ could take place without pain, yet, as is well known, the liver, lungs, kidney and some other internal organs can be cut or in many ways injured without pain, and is it not possible to develop the use of narcotics, to the injury of the whole system, without pain?

Again, as Witmer says,[1] "To assume that every sensation or mental state, whatever, may be presented in the extreme of pleasure, in the extreme of pain, and in any pleasure or pain intensity between these and indifference, would require that quinine, in proper intensity, should give a pleasure equal in intensity to that of the exercise of the sexual function; that the odor of violets, in sufficient intensity, should give a pain as decided and intense as the agony of angina pectoris."

It does not seem to me that this theory can be made to account adequately for the different facts of pain, nor does it seem to me that pain is simply the opposite of pleasure. Real pain often seems as distinct in itself as any other sensation, while pleasure never seems distinct from the sensations or associations which produce it.

Among those who hold the view that pain is a sensation mediated by specific nerves of pain, Prof. M. von Frey of Leipzig has given, perhaps, the best experimental proofs, while Dr. Herbert Nichols of Harvard has presented, I think, the most plausible theory, based on the assumption of specific pain nerves. As these two views seem to represent pretty clearly this side of the subject, I shall give a brief synopsis of both.

Von Frey gives the result of his experiment in two articles, the first published in July and the second in December, 1894.[2] He used as apparatus a great number of light sticks, 10 cm. long, to the end of each of which was attached a hair, forming a right angle with the stick. These hairs varied in length from two to three centimeters, and in size from the downy hairs of a child to the firmest bristles of animals. By a very delicately poised scale, all these hairs were graded, with reference to the force necessary to cause them to bend. A section from the end of each hair was measured under the microscope to get the area over which the pressure was exerted.

With this series of graduated hairs, von Frey could reduce all his measurements to a common unit, which he did in terms of grams per square millimeter; *i. e.*, the number of grams pressure necessary to produce a sensation of touch or of pressure when the contact surface equaled one square millimeter. With this carefully prepared apparatus, he mapped out by a series of pressures the

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[1] *Journal of Nervous and Mental Disease*, April, 1894, p. 213.

[2] "Beiträge zur Physiologie des Schmerzsinnes," Ber. d. math.-phys. Klasse d. königl. Sächs. Ges. d. Wissenschaften. Leipzig, July, 1894; December, 1894.

touch threshold, as well as that for pain, for different parts of the body, giving his results in $\frac{\text{gr.}}{\text{mm.}^2}$. He finds the lowest threshold on the cornea and conjunctiva where he gets pain only. He says: "The hairs exhibit the most sensitive touch apparatus of the body, and the next to the hairs come the hair-bulbs." There are many points in the neighborhood of the hair-bulbs that give only pressure (touch) sensation. Separated from these, and brought out by a greater stimulus, are pain-points. The pain-points are more numerous than touch-points, and are generally found in the places between hair-bulbs. The touch and pain-points are separated by insensitive places. The pain-points, as well as those for touch, vary much in threshold value on different parts of the body. In his first article, he reaches the following conclusions: (1) The punctiform stimulation of the skin, with a gradual, mechanical stimulus, allows the demonstration of two thresholds—a lower one for pressure and a higher for pain sensations. Pressure and pain-points lie locally separate, the former in the neighborhood of the hair-bulbs. (2) There are certain surfaces of the body sensitive to pressure and not to pain, and other surfaces sensitive to pain and not to pressure. The pain-points have, consequently, only a single threshold, which need not lie higher than the pressure threshold of the skin, and may lie even considerably lower, as in the cornea. He concludes, therefore, that the pain sensation is mediated by a special arrangement of pain-points and pain nerves.

The first part of von Frey's second communication is largely supplemental to the first communication. He studies more carefully the relative threshold between neighboring touch and pain-points, and finds the threshold for the latter much higher, except of course on the cornea and conjunctiva, where he thinks there are no touch-points. The relative threshold (pressure threshold, divided by pain threshold) on the arm for mechanical stimulus is given as $\frac{1}{2}$, and on the ends of the fingers as $\frac{1}{10}$ to $\frac{1}{100}$.

The second part of the second communication is the result of a study of these same touch and pain-points by means of electrical stimulation. He finds that touch and pain-points can be located by electrical as well as by mechanical stimulus, the former yielding a whirring, jarring sensation, the latter a pricking sensation. The difference between the threshold of the touch and pain-points is much less when electrical stimulation is used instead of mechanical, or, as von Frey says: "[In electrical stimulation] the threshold of pain-points lies in many places lower than that of pressure-points; the relative threshold is thus greater than 1." There is as much variation in the threshold of different pain-points on different parts of the body as there is between neighboring points of different kinds of sensation.

There is great difference in the inertia of touch and pain-points; the former begin and cease acting quicker on stimulation than the latter. Pain, therefore, accumulates or summates out of proportion to the stimulus, while touch does not. Von Frey has convinced himself that on the extremities 130 shocks per second can be easily distinguished, while 20 shocks per second on a pain-point cannot be distinguished, but are perceived as a continuous sensation. The after-image is, therefore, much more persistent for pain than for touch. The inertia of these points can be shown equally well by mechanical stimulation.

Von Frey concludes by reaffirming his belief in separate pain nerves, with their appropriate end organs. Pleasure nerves he does not assume, but as black is the absence of color, so pleasure is

the cessation or absence of pain.

Dr. Nagel[1] has made a similar study of pressure, pain and temperature points, and from a greater number of cases draws conclusions which are, for the most part, contrary to those obtained by von Frey. Among Nagel's conclusions are the following: The statements of von Frey that the conjunctiva and cornea are capable of painful sensations only, is not correct. The error is explained through the one-sided use of stimulating hairs (which really prick) for the testing of sensibility. By the avoidance of the pricking effects, one obtains pure touch sensations on the conjunctiva. Painless touch sensations are, likewise, easily produced on the cornea under suitable conditions of examination, best through touching the surface with soft, moist and warm objects. Light touches of short duration with the point of a hair are also painless. The physiological proof of pain nerves and of pain-sense organs presented by von Frey, as well for the cornea and conjunctiva as for the body in general, is not made out in a convincing manner.

The finding of spots over the body, where the surface responds painfully to very much weaker stimulus than in other neighboring spots, does not, necessarily, indicate pain nerves and pain end-organs, for, as is well known, the delicate epithelial tissue is covered over with the insensible cuticle. This latter covering consists of scales or cells, which are very much thicker in some places than in others. Where the little furrows caused by the transverse folds of the skin cross each other, we find the most pain-points, as would be expected if these pain-points were due to the absence of protecting cuticle.

Again, the touch-points and the indifferent spots respond painfully if the stimulus is intense enough. This von Frey admits, but says it is due to the disturbing of neighboring pain nerves, which may be true, but the evidence for such an assumption is very meagre. I shall return to this again a little further on.

Dr. Herbert Nichols,[2] on the assumption of specific nerves of pain, proposes a theory for pain on the basis of a supposed biological development.

According to Nichols pain nerves are developed only for the purpose of responding to excessive stimulation, as a warning against violent and injurious influences, and in this they differ from the nerves of other senses, which respond to weaker stimulation and cease acting as soon as the stimulus becomes so intense as to be injurious to the proper functioning of the parts. "The nerves of sight, sound, heat, and so on, would, according to this, respond throughout the range to which they had been differentiated. When the more violent range was reached, which was injurious to them and beyond which they could not perform their function, there the sight would cease, and the nerves of pain would take up the functions to which they had peculiarly developed because of the fact that they could endure them with benefit to the creature from their warnings. Under such an arrangement it would not be necessary that the two ranges, say of sight and of pain, should wholly exclude each other, for it would be well for the warnings to begin before sight was entirely destroyed." Stimuli giving touch and pain, therefore, do not affect the same nerve simultaneously, according to the traditional view, but each sensation is mediated by a separate nerve.

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[1] Willibald A. Nagel, "Die Sensibilität der Conjunctiva und Cornea." Pflüger's Archiv, 5 Feb., 1896, Bd. LIX.

[2] *Phil. Review*, Vol. I., 1892.

This accounts for the frequent separation of pain and touch in consciousness.

As to pleasure, Nichols is not sure that pleasure should be looked upon as a separate sensation in any such sense as pain, and, therefore, pleasure nerves may not exist. However, the pleasures of sex and the pleasures of eating seem to approach more nearly in their distinctiveness the sensations of pain, and may yet be found to be due to specific nerves of pleasure. According to Nichols, pleasure is the primary sense from which all other senses were developed. As each new sense was differentiated, it assumed part of the primary activity of the pleasure sense, which in turn lost just that amount of its former activity. In this way, as the different senses become differentiated, the pleasure sense becomes less and less distinct, and more given to the centro-neural functions. Those senses earliest differentiated would contain the largest element of pleasure—as nutrition and reproduction. "Though the peripheral fibres of our primary sense system have, therefore, been largely submerged and their functions lost, its central parts, with their functions and their particular mental characteristics, have yet been preserved to us essentially unaltered." In another place Nichols says: "The great bulk of our æsthetic feelings unquestionably are associations and of central origin," which harmonizes with his view of pleasure. "It is the ideas associated with the different sense perceptions, and called up by them, that determine them æsthetically."

Nichols would explain the temperature pains by assuming that the pleasure and pain nerves end in different kinds of tissue, which are so constituted that the heat tissues are actively contracting when the cold tissues are either passive or actively expanding, and that the cold tissues are actively contracting when the heat tissues are passive or expanding. "If, now, we assume, by way of hypothesis, that both pain and pleasure nerves also end in each of these kinds of tissue, and that each kind of such nerves is susceptible to a peculiar intensive range of stimulation, we may form an idea of how our common temperature comforts and discomforts may be explained thereby . . . . It may be easily understood, then, how pain, having developed to warn against too intense temperatures, and pleasure, having developed to prompt to certain conduct best suitable to certain moderate degrees of temperature, that they should thus have grown up sensible only to ranges of temperature mutually exclusive of each other."

Whatever may be said of the plausibility of the theories of pain and pleasure based on the assumption of specific nerves, it still remains doubtful whether such nerves exist. In fact, it would become almost as difficult to explain the reason for such nerves as it is to explain the phenomenon of pain and pleasure without them. For it is difficult to understand how so complex an organism, as pain nerves would indicate, could have developed without continual use, and even if in earlier stages of development such nerves did have use and experience, why has nature not tended to abort them during the long periods of disuse? And yet, one must admit that the nervous mechanism seems always ready to discharge in pains, which may not have been experienced for generations and may not be felt for generations to come. Certainly there would be no economy in nature in thus keeping constantly ready a complex mechanism whose office might well be performed by other already useful nerves. Many persons go through life without ever having experienced a pain in certain of the internal organs; and yet no one doubts that such pains might be called forth at any moment by

the right kind of disturbance. If the assumption of pain nerves is correct, why are many organs, like the intestines, liver, kidneys, etc., sensitive to pain only in pathological conditions? Must we assume different kinds of pain nerves? Pain and pleasure are, without doubt, the first conscious activities of life. If, then, their existence be due to specific nerves, it would hardly seem probable that these nerves, the first to function, should have eluded all investigations. Again, if pain nerves exist, we should expect that the exercising of them would increase their sensibility, while the opposite is true, at least, so far as present experimentation has shown.

A touch with the point of a needle in the palm of the hand will often produce, first, a sensation of touch, and, after a second, another distinct, long-continuing sensation of intense itching; on stronger stimulation, there may be first touch and pain, followed by a long-continuing sensation of itching. Shall we, therefore, assume itching nerves and itching points?

Attention plays such a great part in pain that to locate pain-points in the manner pursued by von Frey would require a great many trials with certainty of the spot and constancy of the pain; for, if stimulation of a point was made on the crest of a wave of attention, it would seem more sensitive than another stimulated at a less opportune time.

The assumption of pain-nerves must mean separate end-apparatus, separate paths of conduction, and most likely a separate pain-centre, or pain cells. The existence of such specific neural apparatus is by no means probable.

In reference to those who hold that pain is simply an intense degree of a sensation, and not necessarily an element of every sensation, there are two theories which I shall mention. The first is contained in the exceedingly interesting one of Prof. Z. Oppenheimer of Heidelberg, entitled "Schmerz und Temperaturempfindung."

Oppenheimer begins his treatment of the subject by presenting some of the different views regarding pain and some of the known facts in regard to anæsthetics, inflammations, etc., and then sets himself "to examine how the effect of the pain-exciting influence is brought about, what parts of the organism are concerned in it, with what nerves these parts are connected, and in what relation to the central system these nerves stand."

1. As to the origin of pain. After examining at some length the cause of pains produced by various stimuli—mechanical, thermal, electrical, chemical, etc.,—Oppenheimer concludes that "everywhere in the organs, even in the sense-organs, the real cause of pain is a disturbance of tissue, in particular a disturbance of chemical sort, whereby either the quantity of the newly-formed products of destruction rises above the normal, or whereby products arise through the influence of a foreign body which are not present in the normal condition." There seems to be only one exception to this law, *i. e.*, that of the induction current. In all other cases the tissue appears as the starting point of pain, and can, if one wishes to hold to the analogy of the sense-organs, be regarded as an end apparatus for the sensations of pain. In harmony with this view, pain may be considered to arise whenever an absolutely or relatively too great stimulation for the excitability of the sensation apparatus occurs.

2. In reference to how the nervous connection between the peripheral tissue and the centre is secured, Oppenheimer is of the opinion that this connection is brought about by means of the vaso-motor nerves, in which he includes only the vaso-constrictors and

not the vaso-dilators. He is led to this view by many physiological, pathological and anatomical observations, among which are:

(a) The different sensibility to pain of different organs and parts of the body. In the cuticle, hair, nails, etc., where there are no nerves, vessels or tissues to be disturbed, there is no pain. The intestines, under splanchnic control, the lungs, with no vaso-motor nerves, etc., are, under normal conditions, insensible, but become very painful under pathological conditions. Also those organs whose connective tissue becomes changed into a more or less firm framework and whose circulation is very defective, are normally insensible to pain, but, under a new vascularization which may develop through an inflammatory process, they become quite painful, as shown in chondritis, osteitis, tendonitis, etc.

(b) The close relation which exists between pain and hyperemia. There is one form of hyperemia arising from sectioning of the vaso-motor path—neuroparalytic—in which there is no pain. This indicates that pain fails after an interruption of the vaso-motor path. In the other form of hyperemia, pain occurs so regularly with the hyperemia that it would seem that the cause of both phenomena is one and the same pathological process. "One might think that the centrifugal vaso-motor nerves and the centripetal sensory fibres are stimulated simultaneously in a nerve trunk or in the centre."

(c) The phenomena of hyperæsthesia and of analgesia seem easiest explained by Oppenheimer on his assumption of vaso-motor conduction and separate paths for pain and touch. He says: "We cannot accept the view that there are nerves whose only object is pain. The assumption of pain nerves appears to me to be as unphysiological as if one should wish to assume nerves of sparkling because he once, by a blow on the eye, saw the phenomenon of sparkling. Pain is, as generally accepted, a real pathological phenomenon, *i. e.*, an expression of a physiological function under unusual conditions." In organs where activity causes but slight change, the *feeling* may be scarcely noticeable—*e. g.*, tendons, ligaments, bones. "Pain is not, as some believe, the highest degree of the sensation of a sense-organ, but the most intensive sensation which results in the vaso-motor nerves under the strongest stimulation."

Oppenheimer sums up his view in the following words, which I have translated rather freely:

1. "What until now has been called the sympathetic, consists of two nerve tracts differing completely in their anatomical arrangement. They are distinguished especially from one another in their relation to the ganglia of the sympathetic and in their manner of central distribution.

"The nerve tract known as the splanchnic arises from the spinal cord, goes in the rami communicantes to the sympathetic, the path of which it follows for a certain distance, then leaves it again in the so-called splanchnic roots, without having entered into connection with the sympathetic ganglia, and continues to the abdominal cavity. From what part of the spinal cord it arises, whether it is connected only with the anterior roots, or whether with these and the posterior, is not known.

"The other nerve tract, which may be designated as the real sympathetic, is described most correctly, as it appears to me, if the ganglion of the sympathetic is considered as its place of origin. Disregarding the fibres connecting with higher and lower ganglia, fibres radiate from it in three directions. To it run fibres out of the anterior root, and from it originate fibres which pass through the posterior root to the posterior horn (after having radiated out in

brush-like form horizontally above and below), whence they are distributed partly to the anterior horn and partly to the crossed antero-lateral column. In reference to these latter, it is not certain whether they ascend without interruption to the oblongata, or whether they reach that only after having connected the single segments of the spinal cord among themselves. With regard to the fibres that go to the anterior horn, the assumption can be made with great probability that they connect themselves with the cells of the anterior and lateral horns, from which arise the nerves which pass through the anterior root to the sympathetic.

"Finally, fibres go out from the sympathetic ganglia to the periphery, where they end in a ganglion cell, which gives off fibres for the vessels and for the tissue.

2. "The anatomical arrangement of the sympathetic is different from all other nerves in that the connection of the sympathetic ganglia with the anterior and posterior roots makes possible a centrifugal as well as centripetal conduction, and the forking in the peripheral end makes possible a second arrangement for centrifugal and centripetal conduction, of which the centrifugal serves for the innervation of the vessels, while the centripetal serves for the stimulation going out from the tissue. The single fibre between the sympathetic ganglion and peripheral ganglion cell mediates both kinds of stimulation. There moves along it in an outward direction a continual current, which arises from the anterior horn and the anterior roots and passes by the vaso-motor nerves to the vessels where it causes the vessel tonus. Now, if a current originating in the peripheral ganglion cell through the stimulation of the tissue, and running in an opposite direction should arise, an obstruction will occur in the flow of the first stream, and, in consequence, a loss of tonus will produce hyperemia in the stimulated tissue. At the same time with the obstruction which the centrifugal stream experiences in the periphery, arises a stimulation of the nerve tract, which goes from the sympathetic ganglia through the posterior root and the posterior horn to the vaso-motor cells of the anterior horn and to the antero-lateral columns. The stimulation of this nerve tract announces itself in two forms . . . .

"In the first place, on account of the spreading of the lateral nerve roots in the spinal cord, and on account of the connection which the single segments of the cord possess among themselves, not only the cells lying next the stimulated fibre above and below, but all vaso-motor centres, are set in excitation, and through this, in spite of the peripheral hyperemia, the usual mean blood pressure remains constant. The cause of this constancy of blood pressure—the chemical or physical change in the tissues—is also, taken generally, the cause of the continual excitation of the vaso-motor cells of the spinal cord and of the continuous current going out from the anterior horn, causing the vessel tonus. During life, the metabolic processes are never at rest, and an interchange of rest and activity in the individual organs is ever present.

"The second form of phenomenon is characterized by the appearance of a feeling. How and where this arises cannot yet be told. The course of the sympathetic fibres in the antero-lateral column, their relation to the oblongata, to the splanchnic and to the brain are not known. It is only certain that we have, during the quiet course of the processes of life, an indefinable common feeling (*Lebensgefühl*); that in the activity of the single organs, this feeling becomes more distinct and reaches consciousness as a sensation of the organ, and that with the strongest stimuli that reach the tissue, pain arises. It is also certain that by stimulation or inter-

ruption of the sympathetic path in the posterior horn, vaso-motor disturbances of different kinds present themselves. In the case of stimulation, hyperæsthesia is observed, and in the case of interruption, analgesia.

"The specific effect of the centripetal path of the sympathetic ganglia on vessel innervation and feeling is shown, not only when in consequence of stimulation of the tissue nerves the centrifugal stream is obstructed, but also when the vaso-dilator nerves are in activity and effect an obstruction of this stream. The neuro-tonic active congestion is connected with violent pain.

3. "For the temperature sense we have no special organ as for the other senses. The temperature sensation is rather composed of two simultaneous excitations, one of which reaches the centre by the tissue nerves and the sympathetic and the other by the specific nerves of touch."

As will be seen, this theory is of a physiological nature, basing the origin of pain on a chemical change of the tissues. The theory is unique in its departure from trodden paths, and seems to answer many of the known facts in a very satisfactory manner, especially the pains of fatigue, hyperemia, etc. Nevertheless, the assumption that the tissues act as end-organs for pain and that the vaso-motor nerves form the path for its conduction, would hardly seem probable.

Among those who have written on the different cutaneous sensations, the name of Dr. Alfred Goldscheider holds the first rank. He has touched on the subject of pain in several of his writings, and in his late work, "Über den Schmerz," he has devoted his attention to pain alone. In a former article (*Du Bois-Reymond's Archiv*, 1885; Supp., p. 87), Goldscheider calls attention to pain-points, which led many to credit him with believing in specific nerves of pain; but, contrary to von Frey, Goldscheider believes that these pain-points are not specific organs of pain. He believes simply that in consequence of especially exposed nerve endings, stretched tissues, etc., less stimulation will produce pain in those regions. Pressure-points always respond painfully when the stimulus is intense, but thermal-points cannot be excited painfully. He says: "The so-called temperature-pain is an association of a real temperature sensation with a painful excitation of the nerves of feeling (*Gefühlsnerven*)." The temperature sensation, as such, may be in the highest degree unpleasant, but never goes over into real pain.

As to the nature of pain, Goldscheider reiterates his former view: "that the sensation of pain is peculiar to the pressure nerves and the nerves of common feeling (*Gemeingefühlsnerven*), but fails in all other sense nerves. Sensations of other sense nerves may be unpleasant, but not really painful." This view is similar to that of Spiess (R. Wagner's *Handwörterbuch der Physiologie*). According to this view, pain is a special quality of the sensation and not a modification of the sensation common to every different quality.

Pains from inflammation are thought by Goldscheider to be due to increasing pressure, and by Oppenheimer to be due to chemical changes and chemical products. As to the origin of pain in general, Goldscheider believes it to be due to a process of summation in the gray substance of the spinal cord. Ch. Richet showed that a series of homogeneous shocks will cause pain, when the shocks singly are too weak to be perceived, and Naunyn found similar results, as already mentioned.

Goldscheider accounts for the phenomenon of summation and secondary sensation by assuming, in harmony with Funke and Wundt (*Phys. Psych.*, 4th ed., Vol. I., pp. 111, 112), "that the sensory

paths are divided in the spinal cord; the excitation runs along the posterior column to the conscious centre, probably only once or twice interrupted by ganglion cells; and, on the other hand, it hits on the way the collaterals of the gray substance, and this not only conducts the excitation, but also brings about a changed excitable condition in the cells. These, after more stimulation, will give forth the stored-up energy, which will likewise be conducted to the sensorium." Two possibilities are here present, "either the pain stimulus is conducted through the gray substance to an especial pain centre, or the path through the gray substance possesses itself the condition which allows the increasing of the excitation until it becomes painful." Goldscheider, like Wundt, holds to the latter view. According to this view, the same peripheral nerves that conduct the heat, cold, or pressure impulses, also conduct the pain impulse, or, perhaps, rather the excessive stimulus which is to produce pain. When these impulses reach the cord, they find separate paths, a primary path through the white fibres of the posterior column and a secondary path through the gray column. Impulses of moderate intensity are conducted along the primary column, but when the impulses are intensive, they overflow through the gray column, where their progress is retarded.

Goldscheider believes, as shown by H. Head (*Brain*, 1893), that pain is due to an increasing sensitiveness in the spinal ganglion cells. Here is where the real change in the stimulus that produces pain takes place. The real source of the pain, then, is in the ganglion cells of the spinal cord. There are two conditions which call forth the increasing excitableness (hyperalgesia) of the sensory spinal cells: the falling out (*Ausfall*) of the spinal paths of conduction, and autochthonic irritability. Goldscheider's position seems to be that all impulses of pressure, for instance, whether excessive or not, are conducted along the same peripheral nerves until they reach the posterior horn, whence, under normal conditions, they pass up the posterior column to the sensorium unchanged, and are perceived as pressure; but, if the impulses are excessive, or the conductivity of the posterior column is weakened, they are conducted by the collaterals to the gray column, where they undergo a transformation, due to the stored-up energy of the ganglion cells, and on account of this change are conducted to the sensorium as pain impulses.

Pain is thus a new element added to other sensations. It seems right to say a new sensation, but as both paths may be conducting the impulses together, it may not be best to look on pain as a separate sensation. According to this view, real pain is due to excessive stimulation of the pressure or common feeling nerves, or to a hypernormal condition of the gray substance of the cord, and, therefore, might better be called an intensive degree of a sensation than a quality.

The different qualities of pain are not due to the pain element itself, but are due to the different sensations with which the pain element is constantly mixed. Pains are of one kind, but are colored by the tones of other sensations. In reference to heat pain, Goldscheider believes it to have no more to do with the temperature sense than the pain which occurs on cauterizing the tongue with sulphuric acid has to do with the acid taste which appears alone with a weakened solution of the acid. It is, however, a pain of the nerves of common feeling through the influence of different temperatures accompanied by maximum sensations and peculiarly colored by them.

"To the cutaneous pains are added the character of sharp locali-

zation; to the pain of muscles is added a diffuse spreading and a feeling of depth, because the overlying skin is perceived as free of pain. The joint pain is frequently connected with a feeling of warmth along with a feeling of stiffness, because the least movement in the joint increases the pain." Along with each pain, therefore, there are other sense contents which give to the pain its peculiar coloring. Goldscheider follows Erb's classification of the different pain qualities, according to which the pains are determined: (a) Through the continual mixing of sense perceptions, as burning, itching, etc., pains. (b) Through the localization and diffusion, as aching, piercing, etc., pains. (c) Through the change of exciting process, as throbbing, etc., pains.

Under the heading of "Pain as a Symptom of Diseases," Goldscheider makes three classes: First, the real pain sensation (*echte Schmerz-Empfindung*). This belongs only to the nerves of common feeling and the pressure nerves, and is brought about by real pain-exciting stimuli (mechanical, chemical, thermal, inflammatory and toxic pains, etc.). In the second class of pains there is present abnormal excitement of the nerves, but not so intense that it should lead to such pain. Sensations, in themselves not painful, by their continuation or their occurrence in unusual places are clothed at first with a feeling of annoyance, and later become painful. This second class is designated as pain of discomfort (*Schmerzweh*),—*dolor spurius*, unreal, indirect, or pseudo-pain. Many of the pains occurring in sickness are of this sort — most headaches, many stomach pains, etc. They are more oppressive and tormenting than the real painful pressure, or tension sensations. In both these classes of pain there is some local suffering of the nervous system, but the mind is not affected. It is entirely otherwise with the third form of pain, which rests on an abnormally increased central excitability, a kind of psychic hyperæsthesia. It is "a 'psychic,' or better, an ideal (*ideeler*) pain." This hyperæsthesia of sensitive mental activity, we find designated in neuro-psychoses as neurasthenia, hysteria, hypochondria, etc. It is developed mostly on the ground of heredity, or from an existing disposition due to long sickness, mental overwork, continuous anxiety, etc., all of which bring about a changed psychic activity. Hypnotic and hallucinatory pains are treated under this head. The physical and psychic pains are not antagonistic. They do not exclude each other, but, on the contrary, may combine with one another. Also a real (physical) pain by frequent repetition, or by long duration, may cause psychic hyperalgesia. The above seems to be an important as well as correct classification of the pains occurring in sickness.

Goldscheider's theory of pain is more attractive to the writer than any of the others presented. It may be necessary to modify his view that temperature points are analgesic in order to explain those tabetic cases where there is hyperalgesia to temperature without hyperalgesia to touch. (Starr: "Familiar Forms of Nervous Disease," pp. 173-175.) It cannot be said to be final, as there are still some facts not answered by it, but it avoids many difficulties. By it, analgesia, whether the result of anæsthesia, hypnotism, or hysteria, is easily explained, for anything destroying the collateral fibres, or lowering the excitability of the gray substance, would destroy pain. It is easy to see how a lesion of the cord might cause anæsthesia or analgesia, depending on the location of the disturbance. Also, how in visceral disturbance, etc., the pain may be referred to peripheral parts of the body, these peripheral parts having their nerve roots in the same segment of the cord as the part of the internal organ disturbed. The secondary sensation, as well as in-

creased and decreased sensibility, can be better explained by conceiving the source of pain in the gray substance of the cord.

As Goldscheider suggested, one may laugh at the idea of the pain of an ingrowing nail being located in the cord, yet the sensory nerve fibre which passes from the toe to the posterior root of the cord, is simply the prolongation of a nerve cell situated in the posterior horn, a structure which acts as a unit. Everybody knows how frequently a disturbance of the stomach may produce pains in the head.

It seems rather unfortunate to be forced to make so much of the nerves of common feeling, nerves which are almost as vague and indefinite as the hypothetical pain nerves. They seem to be the scapegoat for all excitations which can not be accounted for through the nerves already known.

To the writer the distinction between real, direct pain produced by artificial stimulation, local inflammation, neuralgia, etc., and the general pains of discomfort, caused by disturbed nutrition or circulation and general change of nervous functioning, etc., seems to be an important one. The real pains seem a totally disparate sensation. This view would harmonize with the manner of their production in the gray substance as well as agree with their separation in time from touch and other sensations on the way to the sensorium. Real pain cannot be said to belong to the higher senses— sight, hearing, smell, taste,— and cannot properly be treated as the opposite of pleasure. The pains of discomfort, on the other hand, seem to me to be fairly the opposite of pleasure, and properly considered as the feeling-tone of the sensation. They belong to all sensations, and are made up from the complex half-unconscious sensational and ideational elements brought to consciousness along with the sensation.

Both pleasure and the pain of discomfort are general, diffused and complex in their nature, while real pain is definite, generally well localized, and simple. Attention and association, as well as the mental and bodily states, play here an important rôle, and the theories making pleasure and pain in this sense opposite *qualia* of a sensation may not be far wrong.

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II. NEUROLOGICAL.

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The Growth of the Brain. A Study of the Nervous System in Relation to Education. HENRY HERBERT DONALDSON. The Contemporary Science Series. Walter Scott, London, 1895, pp. 374, 77 illustrations and 64 tables.

But a single trial is allotted to each to develop a "sound mind in a sound body." A realization that to the attainment of this end some knowledge of the laws governing the growth of by far the most important organ concerned, the brain, might be of service, has been rapidly dawning, and the present book has been awaited by a goodly audience who are interested in the problem of highest development. Physicians, teachers and parents, whose needs, as stated in the preface, the author has in view, certainly form a large proportion of society.

No book in any language attempts to cover the field as this one does. What Exner's "*Entwurf zu einer physiologischen Erklärung*

der psychischen Erscheinungen" does for the individual with regard to certain types of psychic activity, Donaldson would do for the larger problems of individual development, bodily, mental and cerebral, for differences between individuals, on their physical and mental sides, and for the development of differences of sex, race and civilization. Psychic differences between individuals and races seem to be vast. To what in the brain, especially, and in the body, generally, can these be correlated? To what degree is it possible to control, during periods of growth, the development of the physical *basis* in such wise as to raise the individual to a higher level? The book tries to answer these questions, not with vagaries, theories, or superficial platitudes, but with all the well authenticated facts of neurological science up to date.

In beginning with the egg cell and devoting three chapters to the growth of the body and its single organs, the book may remind some of a certain history of New York, which begins with the creation of the world. But in this connection it must be remembered that any discussion of the brain, apart from its relations to the other organs, is misleading in the extreme, and the emphasis which this treatment gives to the point is valuable and timely. Throughout the book, in fact, everything is referred back to the cell as ultimate unit of structure and function. This is in line with the newest physiology (Verworn, 1895), and gives a feeling to start with of beginning with a solid foundation. In these chapters the brain is compared with other organs as to initial size and relative growth, and the fact of interest to theories of education is emphasized, viz.: that of precocious development of the central nervous system. At birth the brain is by far the largest organ in the body. At the ages of seven for girls and nine for boys, it attains practically its full growth. The percentage of brain to body-weight at birth is 12.81, and the more rapid proportional growth of the body lowers this percentage to 2.23 at the age of 25.

The three succeeding chapters (IV to VI) give a most convenient analysis of brain-weight. Each element from specific gravity and water content to the weight of each part for different ages, sexes and races is carefully discussed. Wherever possible, comparisons and statistics are presented in curves and tables, whose meaning can be readily grasped at a glance. All society is laid under tribute so far as available from microcephalic idiots to the most eminent men. It falls out that the latter have, on the average, somewhat larger brains than the average of the pauper and defective classes, from which our statistics are derived. In a list of forty-five eminent men, however, we note that the brains of twelve fall below the average for common men, 1,375 grammes, and four of these even fall into Topinard's class as "small," 1,250-1,001. Between the different classes of society, the well-to-do and the less favored, considerable difference in brain-weight exists in favor of the well-to-do. No statistics exist by which the brain-weight of eminent men can be directly compared with their neighbors of similar social standing. Hence, the author is compelled to leave us in a condition of "healthy skepticism" as to the main question whether brain-weight has any definite relation to intellectual power.

From the side of gross anatomy the author naturally turns to a study of the structural elements, the nerve cells and fibres, discussing in turn their general characters, their development, the architecture of the adult nervous system, and the relations of structural elements to one another. The reader will find here outlined results of the most recent investigations stated in concise form and well illustrated, but the details are too numerous to follow in a review of

reasonable limits. No dogmatism is indulged in, and each view is allowed to stand on its own merits where difference of opinion exists among authorities. With regard to the question of continuity or contiguity of processes, the balance of present evidence is struck in favor of the contiguity theory. To the fissuration of the cortex as an index of intelligence is given no support, and none to the "criminal type."

Chapters XIII to XVII are devoted to treating the functional side of the problem. A fair statement of localization of function is first made; then the general physiology and physiological rhythms of the nervous system are treated, and continuing with a chapter on "Fatigue," the section closes appropriately with a discussion of changes occurring in old age. The topic of sleep is handled in a helpful manner, and the fact of its great importance, not only as to amount, but also as to the length of sleep periods, is discussed. Recent experiments have proved that "continuous loss of sleep is far more rapidly fatal than starvation, and the final changes are very marked, especially in the nervous system." In old age the brain decreases somewhat in size with the general decline of bodily vigor. According to the curves (p. 325) which the author derives from the statistics, this decline in weight is seen to begin for women at about forty-five, for men at fifty-five, and for eminent men as late as sixty-five. This is not so interpreted as to furnish support for any large generalization, since the data for comparison with a similar number of well-to-do though not eminent men are entirely lacking. A short paragraph, giving the author's conclusions from present evidence, is as follows: "The old age of the central system is in a measure independent of the degree to which it is exercised, unless the exercise be so excessive as to cause continual and extreme exhaustion. So far as known, the lumbar enlargement of the sedentary student does not grow old faster than that of the professional runner, and on the other hand there is no evidence to show that the best exercise of the hemispheres does clearly postpone in them the involutionary processes."

This closes what may be termed the practical part of the book, as distinguished from the two brief concluding chapters, which review the chief facts in their theoretical relations. Before going on to these a few points with regard to the preceding chapters may be noticed. Within the brief space of these chapters we have the essence of the best work of nearly 200 specialists. Scattered as it is through the literature of several languages, it has been a labor of years to bring the data together. More than this, methods of different investigators have varied to such an extent that their results, however good, have not been comparable. The author has spared no pains in working all these results over to a uniform comparable statement, expressing all relations of number and quantity in terms of the metric system. Where page after page of tables occur in the original, he has condensed the whole, wherever possible, into the form of curves, whose meaning is obvious. At points where the work of others has proved faulty, especially in matters relating to brain-weights, Donaldson has been enabled, from results of his own researches, to make important corrections. The author is primarily an anatomist and statistician; wherever possible every series of facts is expressed in clearest possible mathematical terms. This will make the book exceedingly valuable to specialists for reference, because in many instances results are stated much more clearly than in the original paper. A further service has been rendered by the author in the way in which he has effected the total separation of the facts each investiga-

tor has contributed from the prejudices and opinions which he may have entertained at the time. Thus far it is a book of facts and details, well authenticated, and entirely free from all personal coloring. Each table, each figure and every authoritative statement is given its exact reference to authors concerned; and complete indexes of both authors and subjects make it easy to ascertain exactly what an author says upon any desired topic. In all these matters of detail, which really determine whether a book is usable or not, the volume leaves nothing to be desired.

The two concluding chapters on "The Education of the Nervous System" and "The Wider View" bring the main facts of neurology into relation: the one to matters of individual education and development, the last to problems of history, race and civilization. The first fact to receive emphasis is that of the precocious growth of the brain. "Long before birth all the cells destined to compose it are already formed," though all have not developed the connections and relations of maturity. And so rapid is its growth after birth and before "formal education" has begun that the author is warranted in concluding that "the act of living is thus the most important natural education process with which the human body has to do." Thus, throughout the argument, the tendency is strong to limit the efficacy of "formal education" and emphasize the importance of natural endowment. "Nurture is of much less importance than nature." It will doubtless appear to many that the anatomical side is given undue prominence in determining the career of a man. For example, in speaking of Venn's observations upon the size of head of Cambridge students, which show that on the average successful men have larger heads than others, the author remarks: "The accomplishments of this fortunate group are therefore to be associated with innate capacities, and have small ethical significance; they may be admirable, just as are the paces of a well-bred colt, but the colt deserves no credit for his gait." It hardly seems that the data furnished actually prove the innateness of these "capacities." May they not have been wholly determined by controllable circumstances, both pre-natal and post-natal? The physiological side, scope for free play of unfolding powers, questions of food, general home nurture, habits, attacks of serious illness, even formal training, form a vast background of causes between birth and the beginning of Venn's observations. Until all these have been thoroughly analyzed by modern scientific child study, it would seem, to say the least, premature to settle down upon the cold-blooded anatomical explanation. If fate by an iron-handed disposition of nerve cells has predetermined the future of an individual, why attempt to do anything? At any rate the anatomical explanation should not be entertained until every possibility of the physiological has been exhausted. It is certainly a far more open hypothesis to suppose that function determines form rather than that anatomical structure is the prime factor. The author's view is not, however, wholly unmitigated. As he expresses it in another place, "while it must fail to produce fundamental changes in nervous organization, education may to some extent strengthen by way of exercise structures already formed, and also awaken into activity dormant cells." This view carried out to its logical consequences and applied to the whole of life would amount to the physiological explanation. Throughout the chapter, however, the anatomical argument receives all the emphasis.

The problem in "The Wider View" is that of education in its most general form. How is the individual to make the best use of his own limited life-cycle while keeping in mind the responsibilities

of the individual to the race? All the data at our command conspire to prove that civilization has had little or no influence upon size, form or structure of the brain. It is true that Europeans have slightly larger brains than savages, but races exist in which the brain is large and still no progress toward civilization has been made. Disappointing as this may appear to civilized conceit, a plausible explanation for the fact lies near at hand. A savage is obliged to meet all the requirements of his life by his own efforts and his own ingenuity. By mutual interdependence and coöperation, civilized society is enabled to accomplish much more, with possibly no greater stress upon the individual. Conditions of life in a civilized community are more favorable to acquisition of knowledge; "but wisdom, as heretofore, continues to linger, and still to occupy its place as the rare performance of a balanced brain."

The best service of the author remains to be noted, viz., that of clearing the rubbish off the field, of drawing sharply the line between fact and hypothesis. Upon nearly every page he is enabled to tell us thus far our knowledge reaches, and no farther. To carry it further we must look to future observation and experiment. This is a great service indeed.

Ueber die sogenaanten Granula der Nervenzellen. FRANZ NISSL. Neurologisches Centralblatt, 1894, pp. 676-85, 781-89, 810-14.

Ueber die Nomenklatur in der Nervenzellenanatomie und ihre nächsten Ziele. FRANZ NISSL. *Ibid.*, 1895, pp. 66-75 and 104-110.

Mittheilung zur Anatomie der Nervenzellen. FRANZ NISSL. Zeitschrift für Psychiatrie, Bd. L, p. 370.

As the result of a long and patient series of investigations upon the minute structure of the nerve cell under various conditions, we have had occasion to thank the author for the perfection, at least, of two important methods for staining the nerve cell. His magenta method for staining cortex, after hardening in alcohol, gave results of great elegance, and is still useful. It has, however, been superceded by his methyl blue staining for all cases where precise granular staining is desired. This method rests primarily on the discovery of Ehrlich that methyl blue has a selective action on nerve tissue. Under Nissl's further direction it has been possible, by the aid of this selective action, to stain portions of the nerve cell protoplasm in a manner characteristic of different types. Thus, his end result is a classification of nerve cells chiefly by the granulation of their protoplasm. More exactly stated—since Nissl would have us do away altogether with the indefinite word, "granule," — substances which have a special affinity for the stain are deposited in a characteristic manner in different parts of the cell protoplasm and in the nucleus. Thus, according to the condition of the staining, whether dense, light or medium, a cell is said to be in a "pyknomorphic," "apyknomorphic," or "parapyknomorphic" condition. Instead of classifying cells by the number or character of their processes, as has been quite generally done, Nissl would classify them by the characters of staining of nucleus and protoplasm. His classification, as far as we have it, may be briefly given as follows: 1. Cytochrome cells, nucleus not larger than that of a leucocyte and cell-body scarcely discernible, found in granular layer of cerebellar cortex and elsewhere. 2. Karyochrome cells, with nucleus larger than that of glia cells, but only traces of cell-body. Typical examples are found in the cells of the substantia gelatinosa of the spinal cord. 3. Somatochrome cells, constituting the great majority of nerve cells, are characterized by a cell-body of definite contour, which completely envelopes the nu-

cleus. According to structure, these fall into four main groups: (a) arkyochrome cells, in which the stained portions take the form of a network; (b) stichochrome cells, stained matter in rather straight stripes or rows; (c) arkyostychochrome cells, in which both network and stripes are present; (d) gryochrome cells, in which the stained material takes the form throughout of small granules. Figures of all but the latter form of cell may be found under the first reference cited.

On Some of the Newer Aspects of the Pathology of Insanity. W. LLOYD ANDRIEZEN. Brain, 1894, Part LXVIII, pp. 549-692.

The idea underlying this paper seems to be the practical laboratory demonstration of the often repeated thesis that for every psychic fact there is a concomitant physical equivalent. In making his demonstrations, the author follows out the physical details much farther than any other writer with whom I am familiar. The aim of the writer being to present a picture in detail of the deviations from normal to be discerned in the brains of the insane, the background upon which he draws must be naturally the normal organization of the nervous system. This presentation of the normal side occupies about two-thirds of the paper. To illustrate the character of the changes found in the insane brain, the author chooses alcoholic insanity as a type with clear causation, ascertainable beginning and duration, and a type of which abundant clinical material may be obtained. The whole discussion is minutely divided under some seventy headings, and possibly a better idea of its general scope could not be given than by naming over a few of the most important topics in the order in which they are treated. First comes a discussion of older views. Then follows a section on comparative neurology, the cortex and cortical lamination, its different classes and systems of cells, its regional differences, its type in the amphibian, reptilian and mammalian brain, functions of the different cells and layers as revealed by their forms and relations of their component cells, and as indicated by stimulation experiments, and by the phenomena of the epilepsies,—Jacksonian and psychical. From these heads we gather evidence which is taken by Andriezen to indicate that the "ambiguous" cells of the second layer and the long pyramids of the third layer are the cells first to receive incoming impressions, hence the primary sensory cells of the cortex, and that the lower layer of polymorphic cells, last to develop and most fully developed in the human brain, are associational in function. Following with "quantitive" and "qualitive" evolution of cortical elements, their "physiological elaboration," "education," "language," and "mental evolution," the author outlines very clearly the "law of psychogenesis." This is the usual conception that as more and more nerve cells (Andriezen uses the term "neuron" in the sense of Schäfer's preferable English equivalent, nerve cell) are developed in the sensory motor arc, psychic activities rise to higher and higher complexity. Even in a frog's spinal cord, this approaches a point where it has proved difficult to say whether the action is purposeful or purely mechanical. The cortex, according to Andriezen, is an enormously complex growth of "neurons" in connection with the olfactory, optic, and fillet radiations. His scheme is, therefore, the one usually adopted in neurology, with the function confidently asserted for a good many structures about which most other authors remain in doubt. For example, Andriezen treats as an established fact the theory that the dendrons are the receiving poles of the cells, and this becomes a point fundamental to his pathological findings, as we shall see

farther on. He also seems to adopt without criticism the idea often expressed by English writers that the fibre-plexuses in the cortex are the chief seat of mental processes, while the nerve cells are merely nutritional foci which keep the fibres in functional condition. From all that we know of the comparative resistance and fatigability of the nerve fibre and the nerve cell, there seems to be little enough ground for any such conception. All our facts point to the neuron as the conductive part of the nerve cell. Whether the dendron has any function of this sort is still a matter of heated controversy among high authorities. It is certainly a legitimate hypothesis to suppose that incoming impulses may be beating upon, say the auditory centres, with equal force during sleep and waking, and that the response which these arouse depends not at all upon the nerve fibre-plexuses, but upon the condition of irritability of the protoplasm in the nerve cells themselves. So, further, the author boldly asserts that no continuity exists between nerve cells, while Golgi and Dogiel both demonstrate such continuity. The importance of these points will become evident as we pass on to Andriezen's scheme of cortical pathogenesis. And they can be determined, not by dogmatic statement, but by decisive preparations, and these which Andriezen brings forward do not fairly clinch with those upon which Golgi founds his view.

Turning to the pathological side, as illustrated by alcoholic insanity, we find, under the "generalized and extensive type of onset," seven distinct elements composing the symptom-complex. Abbreviated from the author's statement, these are as follows: 1. Diminished power of recollection. 2. Diminished power of attention and volition. 3. Diminished initiative. 4. Diminished muscular power, tremor. 5. Blunting of moral sense. 6. Insomnia, nutritive break-down of cortex. 7. Disturbed balance of cortical representations, both as to the external world and the ego, delusions and hallucinations, suspicious, gloomy feelings, etc. Without going farther into detail, it is sufficient to add that for each of these symptoms Andriezen finds an appropriate pathological indication. For difficulty of memory, slowness of reaction, etc., he finds "moniliform swellings" with coalescence of "contact granules" in the dendrons of the first cortical layer. This is accompanied by discontinuity in the staining of neurons. Failure in more distinctively psychic spheres is accounted for by similar changes in deeper layers of the cortex, and these involve the cell bodies of the polymorphic cells, as shown by "various stages of disintegration," and so on *seriatim*. Interesting and suggestive as all these points are, the one thing lacking is a rigid comparison with normal specimens. Andriezen tells us that his conclusions are drawn from a systematic examinatian of "over a hundred" human brains. This gives ground for some degree of confidence in his results. But we are nowhere told, even, how many of these brains are normal and how many alcoholic, and in how many of the alcoholic his findings occur.

Andriezen's treatment of the authors to which he refers is somewhat loose, to say the least. As a single example of this, I may cite my own case. On page 680 he says: "Hodge's work in this respect, following on the older observations of Sadovski and others, shows" —and so on. Sadovski's paper appeared in St. Petersburg under date, April 17, 1889. My own complete paper bears the date, March 15, 1889, and my preliminary paper on the same research appeared in May of 1888. Further, Sadovski's work, in expressed purpose and method, are so thoroughly pathological as to have only a remote bearing on my own. Then who are the "others"? Personally, I care little for priority, but I would be grateful for refer-

ences to papers touching upon physiological changes in nerve cells prior to 1888. Andriezen certainly cannot have in mind either Anfimow or Pauline Ternowski if he has read more than the titles of their papers.

In this connection I feel in duty bound to add a word of criticism with regard to Andriezen's figures. Thirty-six of these are distributed in the text, covering the ground from the nervous system of hydra to the human cortex. Many look strikingly similar to familiar figures in Golgi, Cajal, Lenhossek, Retzius, *et al.* No credit is given, however, and we are led to suppose that they are all drawn from the author's preparations, or from his imagination. Which of these sources has been utilized is the harder to decide, on account of the difficulty or impossibility of ascertaining exactly how the figures were obtained. In no case is even the magnification exactly stated. No reference is made to the use of the camera, and in no case is an adequate history of the particular specimen given. These are matters of great importance, since the chief scientific value of a paper of this kind consists in accuracy and definiteness sufficient to make either its confirmation or disapproval possible.

The first impression on reading the paper is that a contribution of vast importance has been made. Its failure to bear a rigid cross-examination is, therefore, a keen disappointment. Thirty "General Conclusions," covering over five pages, bring the paper to a close, and, though it is full of suggestion, no squid ever more effectually covered his retreat with a cloud of ink.

III. ANTHROPOLOGICAL PSYCHOLOGY.

By Alex. F. Chamberlain, Ph. D.

The Iroquoian Concept of the Soul. J. N. B. Hewitt. Journ. of Amer. Folk-Lore, Vol. VIII (1895), pp. 107-116.

As the author of this essay is himself an Iroquois, it is a distinct contribution to the literature of pneumatology, such as an educated Indian alone can offer. Mr. Hewitt tells us: "Iroquoian psychic philosophy represented the soul as exceedingly subtile and refined, yet material withal, since it could be enclosed in a gourd bottle; as dark and sombre, like a shadow in color; as possessing the form of the body, with a head, teeth, body, arms, legs, feet, etc.; as partially blind by day, but sharp-sighted by night; as immortal by some, but as subject to death and even annihilation by others; as specifically carnivorous, but also eating the things which constitute the ordinary food of the living; as having the ability of uttering sounds, speech, sometimes resembling the whistling or the trilled note of the cricket, and sometimes resembling that plaintive and doleful exclamation so largely used and imitated in the chants of death and of public and private condolence and mourning." As to the state and condition of the soul after death, "there were several well-defined though inconsistent beliefs." The following soul-words are cited and interpreted at length by Mr. Hewitt: 1. *ěrĭ* (soul, heart, mind, as seat of sentiment), whence comes *wa-kat-er-yoñ'-ta-re'*, "I know it," literally, "My heart or soul is present with it;" (2) *Ka'-ni-kon'-rä'* (soul, mind, intellect), a derivative from the verb-stem *-'ni-kon-ton*, "to think," which itself seems to be a reflexive form of the verb *-kon*, "to see," with "the pluralitative suffix *ton*, denotive of the multiplicity of the act or thing affected by it;" (3) *oñ-non'-kwä't*, which now signifies "medicine," but is in archaic use

for "soul," — its literal meaning, however, is "begging, craving, desiring;" (4) *ug-skĕñ'-nĕ* (soul, spectre, phantom, ghost, death), strictly applied to the sensitive soul and not to the intelligent or reasonable soul; the literal meaning of the word is "bone,"—the primitive Iroquois regarding the "bones" as the soul's abode; (5) *oiä'ron'*, a crystallization of the idea of metempsychosis, for this word, which is also applied to the fetish or symbol of the tutelary spirit of soul of a person, is a derivative which really signifies, "what is typified, copied, imitated in form," etc.; from *oiä'-ron* comes the general Iroquois word for "flesh," *oieroñ'tă'* ("the substance of the soul").

Sexual Taboo. A Study in the Relations of the Sexes. By A. E. CRAWLEY. Journ. Anthrop. Inst. (London), Vol. XXIV (1894-5), pp. 116-125, 219-235, 430-445.

The author has collected from the accounts of travelers an immense store of information, of value especially to the psychologist, on the taboos and prohibitions of sex. The social etiquette, political status, family-life, occupations, religious rites and customs, language, table-manners, etc., of men and women of primitive races in all parts of the world are passed under review, and it is to be hoped that the author, who styles his essay "a preliminary sketch," will soon give his studies some substantial and lasting form. The following sentences are worth quoting here: "The social relations of the sexes have rarely followed the lines marked out by natural laws. At an early stage of culture man seems to have exerted his physical advantages, and to have thus readjusted the balance in his own way. The subjection of the female sex is a general law of history. The inferior position of women does not, however, necessarily involve ill-treatment; which is rare, or unfair division of labor, which has perhaps in many cases been mistakenly ascribed. The main result with which I am concerned is the attitude of superiority assumed by man, and his contempt for woman as a physical and social inferior. The latter opinion of the female sex is the result of subjection, while the feeling that woman is the 'weaker vessel' is universal and may exist independently."

Shamanism in Siberia and European Russia, being the second part of "Shamanstvo." By PROF. V. M. MIKHAILOVSKII, of Moscow. Translated by Oliver Wardrop. Ibid., pp. 62-100, 126-158.

A detailed account of shamanism and shamans among the primitive peoples of European Russia and Siberia, replete with items of psychologic import and value. The training of the priests, the trances, exorcism, and the paraphernalia of the "medicine man" are all treated of, beside legends and folk-lore belonging to the subject. Interesting is the following passage: "Shamanism among the Siberian peoples is at the present time in a moribund condition; it must die out with those beliefs among which alone such phenomena can arise and flourish. Buddhism on the one hand, and Mohammedanism on the other, not to mention Christianity, are rapidly destroying the old ideas of the tribes among whom the shamans performed. Especially has the more ancient Black Faith suffered from the Yellow Faith preached by the lamas. But the shamans, with their dark, mysterious rites, have made a good struggle for life, and are still frequently found among the native Christians and Mohammedans. The mullahs and lamas have even been obliged to become shamans to a great extent. Many Siberian tribes who are nominally Christians believe in the shamans, and have recourse to them."

The Kalou-Vu (Ancestor-gods) of the Fijians. By BASIL H. THOMSON. Ibid., pp. 340-359.

The most interesting portions of this article are those treating of the "journey of the soul" to the dwelling-place of the gods, the mountain of Nakauvadta, the theme of a great drama by a forgotten native poet, and the "new religion," which arose in 1885. This latter, with its "prophet" and its turning of the Bible and the missionary teachings to native account, finds parallels among the Cherokees and other primitive peoples, with whom some clever shaman has seized the resemblance between Bible-story and native-legend to prop up his own power, or to introduce a "new religion." The interest to the psychologist lies in the "ingenious compound of Christianity and heathenism" which these "prophets" put forth. In Fiji, Jehovah and Jesus were identified by Dugumoi, the apostle of the "new religion," as Nacirikaumoli and Nakausabaria, "who, after their defeat by Degei (Satan, the serpent), sailed away to the land of the white men, who wrote a book about them, which is the Bible; only they lied about their names, falsely calling them Jehovah and Jesus." The resurrection and the millennium were prophesied as near at hand, temples were instituted, and the "outbreak of heathenism" was stamped out by the deportation of Dugumoi and the leveling of the site of an entire village.

The Interpretation of Folk-Lore. J. W. POWELL. Journ. Amer. Folk-Lore, Vol. VIII (1895), pp. 97-105.

In this address Major Powell explains in characteristically terse and expressive fashion the various stages into which he classifies the attempts of mankind to interpret man and nature,—*imputation*, *personification*, *reification*, *science*.

The Folk-Foods of the Rio Grande Valley and of Northern Mexico. JOHN G. BOURKE. Journ. Amer. Folk-Lore, Vol. VIII (1895), pp. 41-71.

This detailed study by a competent authority of "folk-foods," contains not a little of interest to the psychologist. It is noteworthy that both the Jesuits and their predecessors in New Spain, the Franciscans, "gave earnest attention to the study of native foods, and improved upon the cooking of the natives." To the natives of America we owe chocolate, the tomato, and the pine-apple—all of which were known to the Aztecs. "So pronounced," says the author, "is the natural aptitude of the Mexicans in the culinary art that I think it would be a wise policy for the general or state government of that country to institute cooking schools and instruct classes in the chemistry and preservation of foods, with a view to aiding in the future establishment of factories for the canning of fruits, meats and vegetables, or the making of the delicious 'cajetes,' 'almibares,' and 'jaleatines,' which will be referred to in other pages of this paper." In the streets of the town of Morelia the "dulceros" offer to the public no fewer than thirty kinds of candies, and candied fruits are legion. Cakes and other toothsome confections are quite as numerous. The vogue of these to-day is attributed by some to the Carmelite nuns of olden days, who helped along nobly the "sweet tooth" of the native women. Capt. Bourke tells us that "there are very few towns which do not maintain public flower gardens in the main plazas," and other evidences of æsthetic tastes are not absent.

L'Infantilisme, le Féminisme et les Hermaphrodites Antiques. Par HENRY MEIGE. L'Anthropologie (Paris), Tome VI (1895), pp. 257-275, 414-432.

The writer of these interesting articles points out the recentness of the terms *Infantilism* and *Feminism*, the corresponding French words "finding no place in the Dictionary of the Academy in Littré, or in any of the medical encyclopedias, though the terms introduced by Lorrain have been in use for more than thirty years. Following M. Féré, the author classifies the sex-anomalies thus: *masculinism* (where the secondary sexual characters of the male predominate); *feminism* (where the secondary sexual characters of the female predominate); *androgynism* (mingling of the secondary sexual characters of male and female); *infantilism* (preservation of the corporeal forms of infancy). The articles are illustrated, and the author gives details of cases and bibliographical references. M. Féré calls attention also to the marked corporeal and psychical rapprochement of the woman and the child.

The Origins of Invention. A Study of Industry among Primitive Peoples. (Contemporary Science Series, No. xxviii.) By OTIS T. MASON, Curator of the Department of Ethnology in the United States National Museum. London, 1895, 419 pp.

The special facilities of the United States National Museum and the author's marked ethnographic skill have enabled Prof. Mason to write an interesting and instructive book of the beginnings of human industry. Under the following chapters: Tools and Mechanical Devices, Invention and Use of Fire, Stone Working, The Potter's Art, Primitive Uses of Plants, The Textile Industry, War on the Animal Kingdom, Capture and Domestication of Animals, Travel and Transportation, The Art of War, the author presents a vast amount of detail useful for the psychology of human action (mental and physical) and the propagation and dissemination of the knowledge *to do*. We have pointed out to us the fore-runners of our modern triumphs of inventive skill, and it is astonishing how many inventions the savage and barbarous races possess. To use the words of the author: "The devices of pristine man are the forms out of which all subsequent expedients arise. The fire-sticks of savages are the earliest form of illumination by friction. The tribulum is the modern thresher with stone teeth. The kaiak furnishes the lines of the swiftest racing boats. The sewing machine makes no new loops. Warfare is still cutting, bruising, or piercing. All art lines and geometry were born in savagery. Society, even, can never change in organizations and motives. Our most precious maxims ante-date literature. The whole earth is full of monuments to nameless inventors." Prof. Mason's book is one that should be welcome to every psychologist and historian of the human mind in its relation to the earth and all that therein is.

The Character and Antiquity of Peruvian Civilization. By GEORGE A. DORSEY. (Reprinted by permission from Denison Quarterly, Vol. III. No. 1, Granville, Ohio), 10 pp. 8vo.

Dr. Dorsey, who has had an opportunity of reasoning *de visu*, since he has been himself in the land of the Incas, takes a very high view of ancient Peruvian civilization, and a very low one of the influence of the Spaniards upon native culture. The condition of the Quichuas "has not improved in a single particular," while alcohol, Spanish oppression, corruption in religion, and the con-

stant reminder of their "inferiority" to the conquerors, have aided in their degeneration. The religion of the most enlightened Peruvians of old was almost monotheistic; they had national songs, love songs, dramas, the best elements of a national literature; as architects and agriculturalists, they surpassed more than one country in contemporary Europe, and in government and social order, their confederation was nobler than those of the Greeks. How far they might have proceeded in culture, had not the irruption of Europeans taken place, we know not; but, as Dr. Dorsey points out, the limited amount of arable land, and the absence of the horse, goat, cow, camel (they had the llama only), forbade their reaching the very highest stages.

The Protohistoric Ethnography of Western Asia. By D. G. BRINTON. (Reprinted May 23, 1895, from Proc. Amer. Philos. Soc., Vol. XXXIV.) Phila., 1895, 32 pp. 8vo.

The conclusions reached by Dr. Brinton in this brief and admirable *résumé* of the results of recent studies of the ancient peoples and languages of Western Asia, are: (1) No evidence of a prehistoric non-Eurafrican race in Western Asia, whose soil has always been held by the Caucasic, Semitic, or Aryan branches of the white race; (2) the area of the Caucasic stock in prehistoric times was more extensive to the south, whence they have been driven by Aryans and Semites; (3) the limits of durable ethnic impressions by the Semites have been from time immemorial the mountains of Amanus on the west, the Masius on the north, and the Zagros on the east; (4) from the Zagros to the Pamir the Aryans (with whom are classed the Medes and proto-Medes) were in possession at the dawn of history; (5) the civilization of Babylonia arose from some branch or blend of the white race, and not from any tribe of the Asian or Yellow Race, still less from the Dravidian or Black Races; (6) the Anatolian group of Asia Minor was allied to the Gallo-Celtic tribes of central Europe, and preceded by probably several millenniums the Hellenic migrations into Asia. Dr. Brinton makes clear the ethnic phenomena of Western Asia, over which imaginative writers have spun so many cob-webs.

Crania from the Necropolis of Ancon, Peru. By GEORGE A. DORSEY. (From the Proc. Am. Ass. Adv. Sci., Vol. XLIII, 1894), 12 pp. 8vo.

Dr. Dorsey gives measurements and tabulations of 123 crania—58 males (natural), 32 males (deformed), 28 females (natural), 23 females (deformed), 14 children. A curious fact brought out is that the average capacity of the deformed males (1,480cc.) is greater than that of the natural males (1,450cc.), while that of the deformed females (1,185cc.) is much less than that of the natural females (1,270cc.).

The Algonquian terms Patawomeke and Massawomeke. W. W. TOOKER. Amer. Anthrop., Vol. VII (1894), pp. 174-185; *On the Meaning of the Term Anacostia*, ibid., 389-393; *The Name Chickahominy*, ibid., Vol. VIII (1894), pp. 257-263.

Mr. Tooker's keen analysis of Algonquian place and folk-names is one of the best contributions of recent years to the science of linguistic psychology. Upon him the mantle of Dr. J. H. Trumbull seems to have fallen. America hardly needs the appeal of De la Grasserie to see the importance to psychology of the study of primitive languages.

What Indians Mean to do When they Sing, and how Far they Succeed. JOHN COMFORT FILLMORE. Journ. Amer. Folk-Lore, Vol. VIII (1895), pp. 138-142.

Prof. Fillmore's thesis is as follows: "I am profoundly convinced that the unity of all music, primitive and civilized, will become the most striking fact which will force itself on the attention of the observer; that it will certainly be found that the Indian always intends to sing precisely the same harmonic intervals which are the staple of our own music, and that all aberrations from harmonic pitch are mere accidents, due for the most part to imperfect training, or rather to the total lack of it." The details of the demonstration of this theory, which seems supported by the experience and experiments of Prof. Fillmore, Miss Alice Fletcher, and Dr. Boas, are given in author's interesting pages.

IV. PSYCHIC RESEARCH.

Apparitions, Thought-Transference and an Examination of the Evidence for Telepathy. By G. FRANK PODMORE, M. A. London, 1895, pp. 401.

Ueber Trugwahrnehmung. Von EDMUND PARISH. München, 1894, pp. 236.

Ueber den Wahn. Von DR. M. FRIEDMANN. Wiesbaden, 1894, pp. 196.

Proceedings of the Society for Psychical Research, ending with and including part XXVIII. July, 1895.

Sphinx. Bd. XXI, 1895. Braunschweig.

The Psychical Review, quarterly, since 1893. Grafton, Mass.

Rivista di Studi Psichici. Anno I, 1895. Milano, Padova.

An American psychologist concludes a series of reviews of recent border-line literature in the January number of the *Psychological Review* with these words: "The telepathic theory, and whatever other occult theories may offer themselves, have fairly conquered the right to a patient and respectful hearing before the scientific bar; and no one with any real conception of what the word 'science' means can fail to realize the profound issues which such a fact as this may involve." He also thinks that "the Sidgwick report affords *a most formidable presumption* that veridical hallucinations are due to something more than chance." A few pages before he speaks of telepathy as a name given "in lieu of a theory about it," which looks as if the issues were not so profound after all. Mr. Podmore, in his "Apparitions and Thought Transference," says "the treatment of telepathy by those responsible for the word involves just as little theory as Newton's conception of gravity." His state of mind seems summarized in the sentence, "There is hardly any longer room for doubt that we have something here which no physical process at present known can adequately account for." This transference, "without word, gesture or conscious thought," and also by channels other than those of the senses, may be in the normal or may be in the hypnotic state. It may appear in the percipient as a vague distress, a blind impulse to act, sleep, hysteria, local anæsthesia, mental imagery of various kinds, ideas, neuroses, and may be the action of mind on mind, or perhaps of brain on brain, etc. "There are indeed indications that contact facilitates the transference." "It is, of course, to be anticipated that the difficulty of affecting telepathic connection would increase very rapidly with the distance." Mr. Podmore also candidly adds that "in our experiments an increased interval between

agent and percipient, especially if a wall or floor is made to intervene, has affected the results prejudicially." "In the experiments conducted in the same room or house, and in most of the spontaneous cases at close quarters, the idea transferred corresponds to a mental image consciously present in the mind of the agent." On the other hand, "in most cases of thought transference at a distance, the idea transferred is not one consciously present in the agent's mind at all—the idea of his own personality." The telepathic junction between two minds may be effected "through the absolute," as Malebranche thought pre-established harmony was, or by means of radiant "neuricity." Mr. Podmore concludes that he is "entitled to suggest that some kind of vibrations, propagated somehow through a conjectural medium, from an unspecified nerve-centre, may possibly explain the transference of thought." That something from something through something, somehow may possibly account for it, is certainly modest, but to our own thinking, does not suggest a very high standard of what scientific explanation really is.

Although "the absence of mundane analogies and the difficulties attending any such explanation yet suggested, forbid us to assume that the facts are capable of expression in physical terms," yet it may be that we have here "traces of the primeval unspecialized sensitiveness which preceded the development of a nervous system —a heritage shared with the amœba and the sea anemone." "There are surely phenomena here which seem to point to super-normal faculties, such as clairvoyance, retro-cognition and prevision, themselves hardly susceptible of physical explanation." In view of all this, it is not surprising that "the future place of telepathy in the history of the race concerns us even more nearly than the mode of its operation," and he proceeds to inquire whether this marvelous new-found "faculty, as we know it, is but the germ of a more splendid capacity, or the last vestige of a power grown stunted through disuse." His conclusion is that while very likely telepathy will amount to a good deal in the far future, it probably played a great rôle in the past, and "is, perchance, the relic of a once serviceable faculty which eked out the primitive faculty of gesture, and helped to bind our ancestors of the tree or the cave in as yet inarticulate community." While "the first stage of our inquiry is not yet complete," and it would be "futile to declare" concerning the new agency, still "if there are sufficient grounds for believing in faculties which give to man knowledge not derivable from living minds, of the distant, the far past and the future, it would be more reasonable to regard telepathy as a member of the group of such super-normal faculties, operating in ways wholly apart from the familiar sense activities, and not amenable, like these, to terrestrial laws."

On the other hand there are things that the psychic researchers have the virtue to doubt. In his article on "Resolute Credulity," Mr. Myers sets forth seven theses with eleven sub-heads, as not yet having received evidence enough to give them even a *prima facie* claim to be regarded as true. These, roughly put, are eastern magic, the Blavatsky performances, influence of the stars, palmistry, the miraculous effect of the water at Lourdes, certain claims of Christain science, save so far as suggestion may account for them, and the production of supernatural or telepathic phenomena, such as are pretended to by some showmen. Mr. Myers explains that there are now two groups of psychologists: first, the accurate experimenters, who work on the senses, fatigue, reaction times, attention, memory, mental imagery, the nervous system in general,

and a host of cognate inquiries. But "the drawback is that such methods and such apparatus are better adapted to give accuracy to facts already roughly known than to carry the inquiries much farther into the depths of our being. It is work preparatory to discovery rather than discovery itself." "At the other end of the range, a group still small, though it spreads yearly, somewhat wider in each civilized land, is attacking psychological problems of the highest importance, but which admit as yet of only approximate and tentative methods of inquiry. This is work of discovery indeed; but it is rough pioneer's work — preparatory also in its own way to the ultimate science to which we all aspire." "If you choose the former task as your own, you can progress without mistakes; if the second, you needs must make many mistakes, since no man who dares not be often baffled can reach the secret of the snows."

To "colligate" the views falteringly shadowed forth above, the agent may, perhaps, act on the percipient by means of a purposive idea if he is near, or a more unconscious "personality suggestion" if far, by an impulse which may be somehow associated with a physical agent, like neuricity or vibrations, or may act independently of these through some medium, or directly mind on mind, or brain on brain, or indirectly through the absolute. The percipient receives the impression either as a mental image, an impulse to act, a vague unrest, or a neurosis. The super-normal faculty in this action played, mayhap, a great rôle in the past, before speech, in binding primitive man into social communities, or, perchance, it is a rudimentary organ of a primitive diffused sensitiveness, which preceded the special senses and even the nervous system. On the other hand, possibly, it is a just nascent faculty whose golden age is yet to come. At all events, in the present time, it is either a vestige or a bud, and we must throughout beware of "mundane analogies," and remember that it is, perhaps, "not amenable to terrestrial laws," nor susceptible of "physical expression."

It would seem that the "time had now come for American psychologists to ask themselves squarely, not in the spirit of "scoffers," but as most urgently needing for legitimate use in research every tentative theory that is scientifically legitimated, whether the above general conclusions of this matter, that "the telepathic theory and whatever other occult theories may offer themselves have fairly conquered the right to a patient and respectful hearing before the scientific bar," and whether there is now "a most formidable presumption" in this field, are right; or whether, on the other hand, as we are profoundly convinced, the entire telepathic presumption is yet very far from being a *prima facie* case, is premature at best, and that it is at present with its rank mazes of mystic guess-work a source of befuddlement and obfuscation galore. To say that telepathy "is a name given in lieu of a theory about it," or, with Mr. Podmore, to say it "involves just as little theory as Newton's conception of gravity," seems to us almost grossly misleading, to say the least. Telepathy began as a definition of a new mode of psychic interconnection, and, instead of resting on the commonest facts of sense, and proving by mathematics, it has yet to find a single fact that can be demonstrated regularly in laboratory courses that proves or even illustrates it with certainty.

First of all, it is the vice of the researchers that they have vicious methods of treating the great body of non-experimental material. The value of the spontaneous cases is exceedingly great, not because they bear on telepathy, but as human documents. Indeed, some of them probably would never have been recorded had not the

society raised the question whether hallucinations were not sometimes veridical. The writer is at present working over 1,700 returns to a *questionnaire*, in which there are, perhaps, two score accounts of dreams of flying, hovering, or floating. These, a dream interpreter might say, strengthened the case for levitation, or, perchance, suggested a vestigial heritage of the time when man's far-off progenitors were aquatic, or would soar through space in the future, or that the soul left the body, and did hover, etc. If our dream philosopher were logically disposed, he would, perhaps, tell us that if we diligently collected cases, we might even reach a degree of probability for something residual about such dreams, as great as Mr. Gurney said existed for thought transference, viz.: "the ninth power of a trillion to one." Now, every logician and every mathematician knows that we have no good theory of absolute chance, and that there is a sense in which the probabilities against any given act or event are infinite, and that the sworn testimony of the four best men in the world, that they really saw the four straight suits of well-shuffled cards dealt one to each man, on an outgoing train from Boston a few years ago, could never begin to offset the vast probabilities against such an event. But our point is that the interpreter's method in treating dreams is not the scientific one, or at least can become so only after a vast collection has been made of all kinds of dreams, and by a method which does not appeal at the outset to the wide popular prejudices that there is something true about dreams. We want thousands of dreams recorded at once by the Nelson or some better method, with all possible detail and circumstances, and then we will study their veridical along with other implications, which is a very different method, and will lead to very different, results than if we had collected dreams of flying or floating to study their bearings on some revolutionary theory of levitation. The dream interpreter might, perhaps, claim great credit as being the first to study dreams, but it is only as the old *natur-philosophie* which held instinct to be divine and prophetic, might claim to have first studied it, when all their work was really only in the anecdote stage. The difference between the methods of the researchers and those of men like Friedmann, or to go farther back, Kandinsky, is immense. The latter wish to know all about the cause, frequency, mental and cerebral conditions and details of all kinds of hallucinations. The former laoded the dice at the start in favor of those that bear on a simply stated but stupendous theory and tremendously reinforced by all the old prejudices that make men lynx-eyed for every faint trace of evidence for the independent existence of the soul apart from the body, and bat-eyed for all against it. It is the difference between Kant and true idealism and Swedenborg and pneumatology. The dreams of a metaphysician are surely not likely to give the true explanation of the dreams of visionaries.

The value of the so-called experimental investigations of the researchers we regard as of far less value than the record of spontaneous cases. The latter will remain a valuable collection of data; the former will, we think, ere long all be looked upon by psychologists as the elaborate tests with the Creery girls have been since their confessions. The writer has visited seers and seances for years and has devised many tests, especially three, which, had they been successful, would very likely have compelled belief in his mind. These involve the strongest possible reproduction in the mind of a past fact of feeling, will and intellection, each one of greatest strength and importance in his case and each also written large on one page, but sealed, the seer to have the package and the writer, at the same

time to intently think its contents. This would, of course, succeed with many subjects, but with the writer it is uniformly declined, or totally failed of. Till there is at least some single case in which with effective apparatus and fixed conditions, telepathy, genuine in quality, but ever so faint in quantity, can be demonstrated as surely as argon can be made, or oxygen frozen, or, at least, as the powers of a lightning calculator, or the hypnotic exaltation of a sense, it must rank with the Keely motor, those who assist at the demonstration of which leave in a deeper limbo of uncertainty than when they went, while only a diminishing few find a *prima facie* case strong enough to prompt the investment of money, unless, indeed, they are already so deeply committed to it that they desperately add more and more in the forlorn hope of saving former investments — a case which we are persuaded has its analogue with some of the researchers. A member of the society who prints communications in the English proceedings, and a person of liberal education, called on the writer a few years since to be shown a special form of the slate-writing trick. This was first done with due talk of spirits, raps, etc., then it was explained and demonstrated. The visitor finally sat back in his chair and said in substance that he must believe that I did it by the aid of spirits rather than as I had explained, for it seemed so much more simple and natural. I could not convince him that I was not a medium, and very probably he thought a most disreputable one, denying the real agency that did my work for the sake of pretending to be scientific. The writer has diligently read the experiments of the proceedings, and can honestly say that there is not one in which the conditions as reported seem to him satisfactory. A physicist may erect effective precautions against one whole group of possible sources of deception; the neurologist against another; the psychologist against another, and so on through a long list; but there will yet remain a vast residual possibility of new codes, conscious and even unconscious, of devices that seem most impressive till known, and then disgustingly simple and even vulgar. The most honest men in the world are often least aware, and that from the very honesty of their nature, of the infinite intricacy of their automatic natures and the tricks it can play. Give us one little fact, ever so little, that we can freely test and reproduce one a year in our laboratory. We will cross seas to see it, will acknowledge our mistaken skepticism, and confess telepathy, and turn the research of one laboratory at least in a new direction.

The following are some of the conclusions reached in recent numbers of the "Proceedings": Applying the new psychological methods to the discussions of the voices heard by Jeanne d'Arc, Mr. Andrew Lang concludes that some persons "entirely sane may be so constituted as to see and hear, as if externally, their ideas and mental impressions," and is also "compelled to believe in some abnormal extension of faculty, corresponding to her nature and unparalleled genius. To a certain extent she was admitted within the arena and sanctuary of the universe." Dr. Ermcova, as a result of experiments with a woman with remarkable automatic powers, especially shown in writing in the waking state, which are described in about seventy pages, sums it all up by thinking that the beginning of an explanation of telepathy, based solely on experience, may be given by admitting the existence of telepathic agents of a nature unknown to us, but certainly different from the personalities treated of in ordinary psychology. These agents, in consequence of a voluntary or involuntary excitation coming from the sensorial agent, transmit to a distance, by pro-

cesses unknown to themselves, the ideas they were charged to convey, or which, of their own initiative, they judge it opportune to convey. The "Elvira" which controls his sensitives he thinks on the whole we may call a personality, and says, "If I had not been previously in rapport with Elvira and asked her aid, it is probable that I should never have had an opportunity of observing a single telepathic dream." Mr. Myers fills nearly 200 pages with the experiences of W. Staunton Moses, "one of the most noteworthy lives of our generation," with whom thirty-eight principal spirits are claimed as communicating, including those of Beethoven, Swedenborg, President Garfield, Louis Napoleon, etc. Mr. Moses, who died in 1892 at the age of fifty-three, was an English clergyman, unmarried, of high character and intelligence, whose phenomena are presented in tabular form, and include movements of objects untouched, levitation, passage of matter through matter, intelligent raps, lights, objects materialized, etc. Mr. Myers cautiously concludes that it remains at bottom a mystery,—that we do not know the laws that govern the distribution of his strange and perplexing gift. "Yet, if indeed through such glimpses, such messages as came to my friend, our race is being obscurely guided into an avenue of eternal hope, it matters little whether we talk of chance or of merit."

Outside England occultism, in the smart new garb of modern science, is abundantly represented. In Germany, current numbers of *Sphinx* discuss the "Determination of Sex at Re-incorporation," magic and theosophy, astrology, speaking mediumship, extra-corporeal existence, the astral body, etc. In Italy the new *Revista* describes apparition at the time of death, translates and digests the English work, and in the opening number last January protests in a long editorial against "misoneism," especially that form of it based on a desire to avoid painful effort and the tedious re-adaptation of the mind to new conditions, which is charged against those who cannot accept the new teluric psychism. The Italian Psychic Society, founded at the end of 1894, had in April, 1895, eighty members. Its object is to have seances, lectures, discussions, and is open to all who recognize the truth of the so-called spirit phenomena, whatever the cause of them may be. Prof. Morenos of Venice argues for will as a force or entity distinct from the other forms of force and able to act against them. Fictitious sensations are proven to be transmitted. The *Revista* shows a tendency to refer to telepathic origin much, often ascribed to spirits, *i. e.*, the knowledge of an automatic writer really does come by telepathy from the things or events rather than from spirits. Spirit photography is regarded as not yet proven. In the *Psychical Review* a professor of physics expresses the belief that "we are very near to a discovery of a physical basis for immortality that will transform most all our thinking." Rev. M. J. Savage says: "Now I know that matter is sometimes moved without muscular contact," etc.; and again: "I have had communications while sitting in my study concerning things that were taking place 200 miles away. Over and over again such occurrences have taken place," etc. Rev. T. E. Allen, first secretary of the American Psychical Society, 1891, intimates that it desires to give a "thorough, candid and sympathetic sifting in this nineteenth century of ours" to "phenomena for which the great claim is made by many that they answer that wail of broken hearts, if a man die shall he live again?" The object of the society, as defined by its constitution, is to investigate "the phenomena of modern spiritualism in accordance with the scientific method." If spirit be a conscious entity dissociated from

matter, and immortality have no physical basis, then, an eminent American professor points out, "the earth would run away from the immortal part of man at the rate of about 25,000 miles per hour," so that spirit navigation would be an act of will instead of being possibly done by gravity without ghostly effort. Psychography, long distance clairvoyance, death prophesied in dreams, the divining rod, inspirational poetry, mysterious music revealed through clairaudience,—such are the themes of the *Psychical Review*, whose editor tells us that he deems himself peculiarly fitted to champion the spiritualistic hypotheses; believes himself, in fact, "inspired" to do it. This shows that although he urges the principle that "no state of consciousness is superior in authority to any other state," he still holds that some men are better for some things than some others — a principle which we venture to believe has much to be said in its favor, even were it a question of authority, for are we seriously told that if only "Messrs. Helmholtz, Huxley, Pasteur and Edison were simultaneously to announce themselves as converts to clairvoyance, thought-transference and ghosts, "there would be a prompt popular stampede that way?" There are proofs "entirely conclusive" for Dr. James that Mrs. Piper has shown in her trances a "knowledge of the personal affairs of living and dead people which it is impossible to suppose she can have gained in any other way. "Such things," he adds, "have broken down for my own mind the limits of the admitted order of nature. Science, so far as science denies such exceptional facts, lies prostrate in the dust for me; and the most urgent intellectual need which I feel at present is that science shall be built up again in a form in which such facts shall have a positive place." For Mr. Myers there is a reality in us far larger than we know, "an individuality which can never express itself completely through any corporeal manifestation." There is always some part of the self manifested, "some power of organic expression in abeyance or reserve."

Now let the reader turn from all this literature with its deep unconscious bias of prejudice, in the form of hunger for immortality, which weights every die of fact, where the atmosphere, though clearing up, is still murky with traces of nearly every form of superstition that the world has ever seen, to a book like Friedmann's on Illusion. The author, though a "nerve doctor" and a good clinician, is chiefly interested in normal psychology and philosophy and logic. His problem and method, which are all we here consider, are as follows: To collect with every possible detail and with as great accuracy as possible good typical cases of illusion, morbid and normal, including those of fabulists and phantastics, and then to study them from all points of view. The eccentric character and the tendency to regard or make them veridical, are parts of the illusion to be explained, and not data on which to base transcendental hypotheses. The structure of the self, the influence of feeling upon the evolution of the more complex illusions, their physical, psychic and hereditary root, are legitimate and right questions. Parrish's treatment, although he does consider briefly, but with negative conclusions, whether hallucinations are ever veridical, is also satisfactory.

If it be claimed that a folk-lorist gathering material from all over the world concerning amulets and charms, deems himself "called" to discuss whether they do really charm, or a student of spontaneous retinal imagery wants to ask if they are visions of things supernal, and claims "fair play" for these superstitions, or if a group of philanthropists wants to establish a mission or university settlement in the heart of ignorance and credulity to mitigate them—

we say merely that the energy could be better spent by better methods. It is curious that epistemologists who think that nothing is real but the thought of the individual at the moment, should feel impelled to "compensate" by holding that what used to be thought dreams, are really veracious of objective things. Is it a useful or a pernicious service to get fetishism, animism, etc., restated in current terms of science? When, if the nerve is severed that goes to my arm or leg, the two severed ends can never be put so near together that a volition to bend the limb can jump across the infinitesimal interval, is it likely that holophrastic impressions leap vast spaces? Again, is there no isolated conductivity among different fibres, or must we go back to the sixteenth century physiological sympathies? Science to-day, to quote a street song, has a great big swallow, but it can't quite swallow some things and continue to be science, and Clifford well said there were some theories a man could not verify without ceasing to be a man. The glory of the new psychology is not so much, as Mr. Myers thinks, that it is more exact, though that is of course true, but that its conclusions are more certain. So far from being less fit to open new fields and make fresh and great additions to the sum of human knowledge, than to make what was before known more precise, its chief claim is, the writer thinks, that it has first erected the ideal of collecting all the typical psychic experiences of man, his feelings, acts, ideals, normal and morbid, child and adult, criminal and law-abiding, and adding those of animals, and using all these as data, not to confirm any old longings or new theories, but for the most objective induction and painstaking study, fully persuaded, meanwhile, that the conclusions, whatever they may be and however long delayed, will be not only larger than all that can be sugared off out of spiritism, but that they will give us a vastly loftier and more adequate notion of all that can be called psychic. G. S. H.

V.—MISCELLANEOUS.

Mental Development in the Child and the Race. PROF. JAMES MARK BALDWIN. Macmillan & Co., 1895.

Professor Baldwin has treated in this book a subject that is new and full of absorbing interest. As the title would indicate, he has tried to do for the development of the child and the race what Darwin undertook for the animal series in his "Origin of Species." He simply applies the principles of evolution to mental development, and shows that what holds good for organic life is true also for mental life. It is in no sense a book for the general public; in fact the style of the author is such that he will probably never become popular with the masses. The book has already been reviewed in the public press several times, and the character of it is pretty well known. The task that remains for the present reviewer is to call attention to the points where the author has succeeded and to point out some respects wherein he has failed. He has so frequently appeared in several prominent journals, discussing subjects relating to children, that when this book was announced, it was looked for with considerable interest, as it was believed that he would carry out in more detail and with greater thoroughness the work inaugurated by Darwin, Preyer, Perez and others in the study of their own children. In this respect the public will certainly be disappointed, for after the first three or four chapters very few observations and experiments made upon his own or other children are given, and the book is devoted almost entirely to theorizing

and speculation. It is true that in many of the fields traversed by the author, no facts are yet established. But, then, why not have waited for them, or given time to gathering them, instead of elaborating theory in their absence?

The book opens with a chapter entitled Infant Psychology, in which there are a number of acute observations about taking up the subject, and the author shows a good understanding of what is to be done and the limitation of it. He next discusses the new method of child study, which is that of dynamogenesis. This is well presented, and the success he attains in using it in the study of color perception merits hearty approval. His criticisms on Preyer are thorough and just. He fails, however, to tell us the kind of colors he used, so that it is impossible for anyone to verify his results. The treatment of right-handedness is full of suggestion, and the author shows himself a master of the method he advocates.

Right-handedness is reduced to a spontaneous variation in the equality of the two hemispheres, and it is shown "that the influences of infancy have little effect upon it * * *"—a conclusion which must seem in the end unsatisfactory. The chapter upon Infant's Movements, in which he treats of tracery, imitation and the reflex movements in walking, is one of the best in the book. Certain details in his explanation might have received more careful consideration, but, on the whole, the chapter is most suggestive and full of interest, and will doubtless lead others to undertake further work in this direction. The use he makes of the child's movements as indicating his mental development, and the great stress he lays upon the motor side, will be the permanent value of the book.

When we come to the chapters upon the Theory of Development and Motor Attitudes, the author becomes entirely speculative, and the book loses the freshness of the first chapters. He takes up the various theories of development and considers them with care and acuteness, corrects and amends them, with the view to rendering them more plausible, broad and adequate. The suggestions he makes and the thoroughness with which he applies the principles of evolution, laying great stress upon "selective reaction," excess in motor discharge, pleasure and pain, heightened nervous discharge, the need for repetitions of stimuli, habit, accommodation, etc., when considered from a purely speculative point of view, will prove most valuable to future workers in the same line. The outcome of these chapters is to show that the principles of organic development are the same as those of mental development. The objections urged against the theories of Spencer and Bain will certainly find acceptance as being valid from a speculative point of view, and, while they are not supported by sufficient facts, there is something commendable in the attempt to find an hypothesis that will be applicable to both organic and mental life. The result will be to show that organic and mental life are one and the same and follow the same law of development. This is in the right direction. But the reader who wishes to find practical suggestions for beginning the work of child study or the study of race development, and hints as to the kind of thing that needs investigation, must feel not a little disappointed, and exclaim after finishing them, "Is this all there is to offer?" What is to be gained, after all, by this playing with theories, and by rendering them consistent and reasonable? The criticism against a theory that it seems unreasonable is not an absolutely decisive and convincing objection, and the contrary is also true that a theory that is reasonable is not certainly right. The author makes the very pertinent remark about the conflicting

opinions held concerning the inheritance of acquired habits when he says that none of them are disproved by fact. The same is true of his own theories; they are not only not proved, but they are not supported by a sufficient citation of facts and investigations. This theorizing is not a fault into which Prof. Baldwin has fallen by accident, but is a conscious and professed purpose with him. He says upon page 37: "Only the psychologist can 'observe' the child, and he must be so saturated with his information and his theories that the conduct of the child becomes instinct with meaning for his theories of mind and body."

Further: "That most vicious and Philistine attempt, in some quarters, to put science in the straight-jacket [the proof-reader should have made this "strait-jacket"] of barren observation, to draw the life-blood of all science—speculative advance into the secrets of things,—this ultra-positivistic cry has come here as everywhere else and put a ban upon theory. On the contrary, give us theories, theories, always theories! Let every man who has a theory pronounce his theory! This is just the difference between the average mother and the good psychologist—she has no theories, he has; he has no interests, she has."

Although I may be called a vicious Philistine, I must unhesitatingly pronounce this poor nonsense. I deny that the aim is to reduce science to barren observation, and I would make it something more than "speculative advance into the secrets of things." Theories are at bottom only working hypotheses, and beyond this they are of little service. This has been Prof. Baldwin's great mistake, and it has rendered his book in some parts a barren waste of speculation. It would have been well, for example, to have given us some facts that would have made it "perfectly certain that two in every three children are irretrievably damaged or hindered in their mental and moral development in school * * *"

In view of all this, it would be well to note the fact that the author tells us "there are only two ways of studying a child, as of studying any other object—observation and experiment." Does he himself not add and make use of a third, namely, speculation, when he says it is "theories, theories, always theories," that we want? All his views are borne out by trends in "current thought," "recent thought," "current theory," "current doctrine," "psychological theory," "biological theory," etc. These words are reiterated until the reader is fatigued. He says that parents and nurses may give results that are of some value, but there is the uncertainty whether they have not been colored by affection, pride, jealousy, etc. Scientific men are not free from affection, pride and jealousy with respect to their children or their theories; they are human beings. Although theories must, to a certain extent, precede experiments, they may vitiate them and lead the experimenter to overlook facts that come to view during the experiment. But since Prof. Baldwin has recently published an extended syllabus calling for general observations upon the social development of children, while he affects to distrust the "anecdotes of fond mothers," perhaps his objections are not to be taken too seriously.

In the chapter upon Imitation we are treated to more speculation. The author complains of the neglect the subject has suffered in "psychological theory," scarcely intimating that it needs "investigation." Imitation is defined as a phenomenon of consciousness, which "is probably never absent from living organisms * * *" It "is an ordinary sensori-motor reaction which finds its differentia in the single fact that it imitates, that is, its peculiarity is found in the locus of its muscular discharge." The first assumption is en-

tirely unfounded except upon the opinions of certain authors, and the second is a bit of wordy remark which adds little to the reader's information. It would have been much better to have cited cases of imitation such as the author must have observed in his own children, and come in the end to his conclusions from an analysis of these. That which the child imitates, "the copy," he says, is clearly defined in the child's mind before he imitates it and he proceeds by reproducing it; the opposite supposition, that by imitation a child clears up his idea of what is presented, can find much support, but he cites nothing to support his view. The whole matter of imitation is left practically where he finds it so far as permanent and established results are concerned, and, although he announces the crucial question involved in imitation, as it seems to me, when he says that it is concerned with a nature and significance of the copy which is imitated, he does not solve the question; he does not show how the copy brings about a reproduction of itself, whether it has the power to coördinate the muscles so as to bring about a reproduction or whether it issues in random movements, which are slowly corrected by comparing the movement with the original stimulus. These questions should somewhere find an answer in the light of observation and experiment and not of speculation and assertion. He approaches this matter on pages 378 and 379, and finally leaves it in a very unsatisfactory way by saying that the child does bring about a change in his reactions from senseless repetition to intelligent conformity to the copy which he imitates, "but he does it, and the least that this can mean is that there is in some way a modification of the impelling influence of his old associations." He shows how memory, association of ideas, assimilation and recognition, conception and thought, and emotion and sentiment may arise through imitation. In this he shows the same acuteness which has characterized his thinking in all other parts, but the confirmation is lacking as before. His suggestions at the end of his treatment of this subject, on how to observe children, refer almost entirely to the child's social surroundings. He notes an especially important point in the influence of companionship.

In the last few chapters the author discusses the rise of volition and voluntary attention, closing with a résumé of the theory of development. We have looked in vain for a thorough treatment of the origin of consciousness, either in the individual, the race or the animal series; it is assumed thoughout the book, and authorities are cited who hold that it is present in all forms of animal life. There is a vague promise in the preface that the matter will receive fuller treatment in the proposed volume of "Interpretations."

Many will find Prof. Baldwin's book stimulating; it clears up one's conceptions of many things and lays a stress upon others that will bring them into greater prominence and make them the subjects of investigation, but the author is not free from the regrettable and too common tendency to emphasize the indebtedness of the subject to his own contributions, and to contend over small points of priority. He picks a quarrel with a certain "well informed" biologist in the note upon page 247 about a small point; he charges Bain with using some of his views, (in Bain's words,) in a note upon page 196 and then quotes several dates,—also in a note upon page 317, to show that he was entitled to the authorship of a view which Ward had expressed, and in another case tells us that his ideas had been thought out several months before they appeared, and thus he was able to antedate his rival.

T. L. BOLTON.

Thinking, Feeling, Doing. By E. W. SCRIPTURE, PH. D. Flood & Vincent, The Chautauqua-Century Press, 1895, pp. 304.

This seems a *ne plus ultra* in the way of popularizing, not to say vulgarizing, laboratory psychology. The frontispiece shows five American flags, as seen respectively by the red, green and violet blind, by totally color blind, and by normal eyes. Chain reaction is illustrated by a group of European monarchs and other dignitaries. The author's pictures and scenes from his laboratory often recur among the 209 cuts, and both are boomed with a sort of Sunday newspaper advertisement effect. All this, with the telegraph and Associated Press comments on petty variations in apparatus, or in their use, and the amazing number of new instruments as shown in the Willyoung catalogue, which emanated from the psychological laboratory, having the largest number of rooms of any in the world, certainly show that the academic study of the human soul has changed since the days of Upham, Hopkins and Hickok. Dr. Scripture wastes no time on such petty matters as form of expression or style. His book, he says in the preface, is an answer to the question once put to him, "Are you not afraid that all this accurate and fine work in the laboratory will scare away the public?" This suggests the question which staid and respectable Christians used to ask of the work of General Booth. But his homely and perfervid zeal had its own place in the world, and so has Dr. Scripture's book. Wundt he speaks of as "the greatest genius in psychology since the time of Aristotle." "No one else," he tells us, "has produced a book explaining the methods and results of the new psychology." "This is the first book on the *new* or experimental psychology written in the English language. That it has been written *expressly for the people* will, I hope, be taken as the attitude of science in its desire to serve humanity."

On the whole, we are not disposed to discuss the book in this light and sneering way, as do other reviewers of it we have seen. There are germs of thought and tendency in it which the "arm-chair" psychologist, to use the author's fit phrase, will do well to ponder. In the first place, Dr. Scripture is as anti-materialistic as they. Again, he well ridicules the current habit of translating mental processes into imaginary brain processes, as if brain dissection could explain facts of mind, or we could have a chemistry of anger. Again, he well says the day of individual systems is past, and it is soon to be as obsolete to speak of anybody's system of psychology as of so-and-so's system of chemistry. Psychology is now, he thinks, simply a great science to which all can contribute, and the day when the writing of voluminous general text-books will rank with painstaking and tedious investigation, or the reading of such books will be considered an education in psychology, is fast passing away. He would not have psychology longer deduced for philosophical prejudices. Both the faults and merits of the book are those incident to youth and to a new subject. Many of his devices in the laboratory, while by no means great inventions, are extremely convenient. Still more are of high pedagogic value in not only the popular lecture room, but in the college class room. The chief value of this little volume is that it is sure to make the teaching of experimental psychology, both in normal school and college, more effective, while for the author's pet foibles of exactness and precision, it is so needed a lesson to the "arm-chair" professors that we almost hesitate to quote Aristotle to the effect that it is the mark of a man unread and immature to insist on treating a subject with more exactness than the nature of the subject requires.

NOTES.

FEELING.

Of the eighteen papers read at the Princeton meeting of the American Psychological Association in 1894, seven were concerned with affective processes,—pleasure, pain or emotion. The technical journals have published many articles upon the same topics during the current year; and a book upon the "Feelings" is promised by Professor Ribot. This preponderance of interest in the affective side of mind is not by any means accidental. It shows that psychologists are realizing (what is indisputable) that experimental investigations of sensation and conation have been unduly preferred in the past, that feeling is not so well understood as association, memory, or attention. Somewhat unfortunately, more work has centred upon emotion than upon the simpler processes of pleasantness and unpleasantness: unfortunately, because to understand the complex, we must previously understand the simple. But this very fact suggests that it may be useful here to consider the defects which reviewers have pointed out in the most recent systematic attempt at a feeling-psychology, — the discussion by Külpe in his "*Grundriss der Psychologie.*"

Külpe recognizes the feeling qualities, pleasantness and unpleasantness, as elemental qualities of mind. His treatment is unsatisfactory in three respects: as regards the relation of feeling to sensation, as regards affective introspection, and as regards psychophysical theory.

1. Feeling may be related to sensation, the author says, in one of three ways. It may be an attribute of sensation; it may be a function of sensation; or, it may be a coördinate process. The first possibility is quickly, and (it seems) effectively, disposed of. The second is also rejected. From the logical standpoint, the reader may well be a little surprised at this. Had Külpe retained the activity consciousness, Wundt's apperception, intact, he would have been obliged to show the impossibility of functional relation between feeling and sensation (or, better, sense stimulus). But, apperception being reduced to something else, there is no logical necessity for the isolation of the two remaining elements: the discussion looks like a survival from a period of thought in which the activity consciousness was admitted to be elemental. To this it must be added that the actual arguments alleged are not entirely convincing; that the "serial method" of affective investigation demands a "certain dependency" of feeling upon sense stimulus; and that some measure of truth is believed by the author to reside in the "peripheral physiological" theory of pleasure and pain.

This difficulty is expressed somewhat differently by Dr. Meumann (*Année psychologique*, p. 511). "It may be questioned," he writes, "whether the three hypotheses formulated by the author are the

only possible alternatives. Could we not think of some other kind of relation as obtaining between sensations and feelings?" Külpe's three relations, indeed, may very easily be increased to five: feeling may be sensation attribute, a function of sensation, coördinate with sensation, dependent upon sensation in some way not functionally expressible, or—itself sensation. Dr. Meumann appears to have the fourth of these possibilities in mind; and it may have been suggested by Külpe's section on the serial method. The fifth we shall return to later.

2. Another point is emphasized by Dr. Martius (*Zeits. f. Psych.*, IX, p. 42). We must not neglect, he says, to analyze feeling, wherever possible, "from mere inner experience." But when we look to see how Külpe conceives of this analysis, we find but scanty indications. The dependency-formula of *sensible* discrimination is:

$$S\,D = \frac{\text{f A. E. P. B.}_1}{\text{g F. H. B.}_2}\ M.\ L.,$$

where *A* is attention; *E*, expectation; *H*, habituation; *P*, practice; *F*, fatigue; *B*, bodily processes; *M*, memory, and *L*, language. The dependency-formula of feeling includes *A*, *E*, *H*, *F* among its factors. But the formula cannot be written out as it can for sensation. Attention to the sense concomitants of the feeling intensifies it; attention to the feeling kills it (§39; 1, 2, 3). The same holds of expectation. *M* and *L* (not involved, however, if the expressive method is followed) are equally equivocal. Plainly, then, there can be no pure formula for any "affective discrimination;" while a mixed formula lays all the weight upon the sensible.

What are the facts? Can we "feel" differently, and express the difference, independently of sensation? If we can, what is the mechanism of the process? Külpe has analyzed sensible discrimination so successfully that our regret must be the greater that he has said nothing upon the point. So much, at least, seems clear,—that the "psychological methods" which he enumerates are not directly applicable to the study of pleasantness-unpleasantness.

3. The third unsatisfactoriness in Külpe's treatment is the obscurity and vagueness of his *theory* of feeling. He would apparently combine the views of Lotze and Wundt, making both more definite. But no definite propositions are offered. Here, however, it is rather our general ignorance of the facts than any cloudiness of the author's thinking which is to blame.

The fifth possibility, mentioned above under 1, was that feeling might *be* sensation. As there are not a few psychologists who favor this view, more or less explicitly, it will not be out of place to state the arguments urged by Külpe against it. They are briefly as follows: (*a*) Feelings have not, as sensations have, any objective significance apart from their subjective or psychological. (*b*) Feelings are far less dependent upon external stimuli than are sensations. They depend upon mental dispositions, which have their history. (*c*) The qualities of sensations are dependent upon the excitation of quite definite peripheral and (probably) central organs. The qualities of feeling evince no determinable dependency upon particular external bodily organs: of their relation to the central organ we know nothing certainly. (*d*) Feeling is blunted by practice and habituation in a way which differentiates it from sensation.

Until these differences have been resolved, there seems to be no choice but to accept feeling as ultimately distinct from sensation in the normal human consciousness.

Bibliographies.

Experimental psychology now possesses no less than three bibliographies. The *Zeits. f. Psych.* has published yearly indices since 1889; the *Année psychologique* and the *Psychological Index* have made a beginning with the literature of 1894. All three are imperfect, so that all three must be consulted; after consultation, the inquirer may look further for himself.

The existence of three yearly indices plainly involves much needless labor and expense. There is, moreover, an especial danger in the present instance. The *Psych. Index* for 1894 appeared in March, 1895. The *Année psych.* for the same year, advertised for March, seems to have been issued in April, 1895. The *Zeitschrift* index for 1891 is dated February, 1893; that for 1892, August, 1893; that for 1893, October, 1894; that for 1894 is unpublished at the time of this writing. It may very well be that the compilers of the latter, trusting that the *Psych. Index* is complete for American titles, and the *Année psych.* for French, will check their lists by reference to the two previously issued indices. Such a course would be most unfortunate. The *Index* is incomplete as regards the United States; while, on the other hand, its first three pages contain two French and two Italian titles (Nos. 22, 35, 46, 50) which do not appear at all in the *Année psych.*

Much better results would be reached by the formation of a central bureau,—and Germany has the claim of priority. For (1) the *Index* could then be made really complete, the compilers of the *Année psych.* being responsible, *e. g.*, for French, Spanish and Italian titles; the compilers of the *Psych. Index*, for English and American, and the German compilers for the rest. (2) International coöperation would render possible a sifting of the titles included in the bibliography, and would thus ensure the elimination of certain grotesque items in the present lists. (3) A less severe draft would be made upon the purse of the individual psychologist. And (4) the way would be paved for the incorporation of the psychological output in the "international catalogue of scientific literature" now mooted in the technical journals. Even if the work required a full year for its completion, proof-sheets, issued to subscribers as published, would be as valuable as the imperfect lists which are at present obtainable in March or April.

If the three indices continue to appear side by side, it is to be hoped that the *Année psych.* will in future refrain from translating foreign titles into French. The proceeding is contrary to every principle of sound bibliography. And as the great majority of the works catalogued have been competently reviewed antecedently to their listing, it is also to be hoped that brief characterizations and appraisements of their contents, over the reviewer's signatures, may be appended to their titles.

The Visual Qualities.

It has been customary to accredit sight with about 41,000 qualities of color and brightness. The scattered statements in the literature seemed to justify the assumption that there were some 800 distinguishable brightnesses between the limits of the deepest black and the most dazzling white; some 200 distinguishable colors in a solar spectrum of average intensity; and some 200 distinguishable degrees of saturation for each of these 200 qualities (not 800, as might be imagined, since brightness discrimination suffers very considerably by the intermixture of homogeneous with the mixed light); in all, 41,000, more or less, and probably more.

Professor König has recently computed the number of discriminable spectral colors, and the number of brightnesses from limen to terminus of stimulation (*Zeitschr. f. Psych.*, VIII, pp. 375 ff.). He gives the former as 160, the latter as 660. If these numbers are correct, the total falls to about 33,000. It may very well be, however, that Professor König has underestimated the number of qualities on the brightness scale.

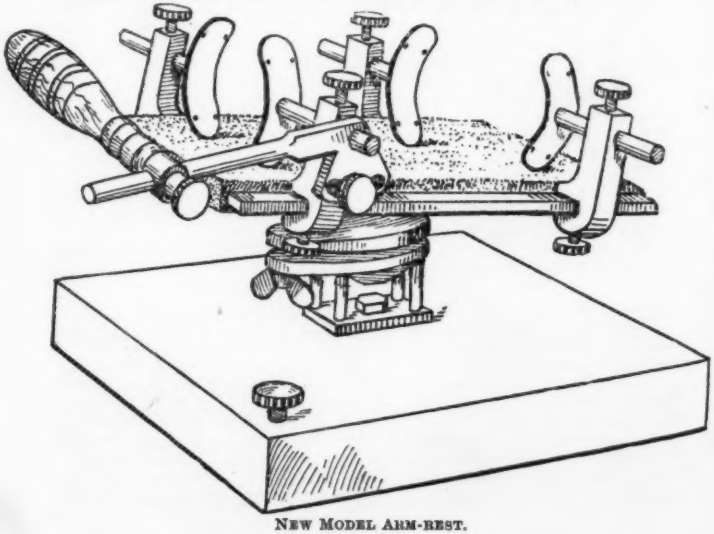
The Spectrum Top.

If a top composed of black and white sectors is rotated at a certain most favorable rapidity and in a certain most favorable illumination, the white clearly takes on the tint of the spectral color series, from red to violet. This fact has been recently discussed at length in various scientific journals; and Messrs. Newton & Co., of London, have even taken out a copyright on the manufacture of the requisite black and white discs for demonstration of the phenomenon.

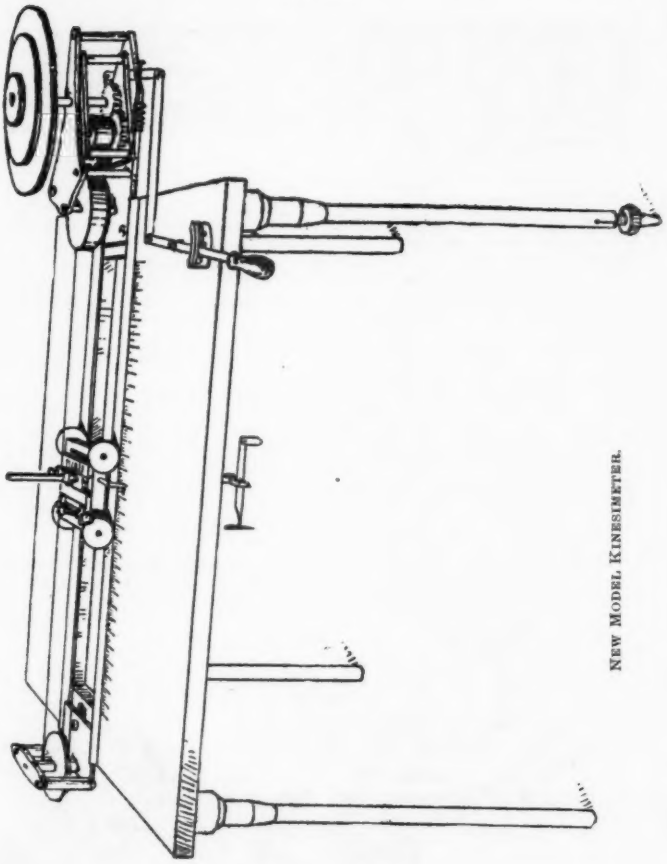
Professor Cattell has pointed out (*Science*, July 5, 1895) that the spectrum top was known to Fechner in 1838, and that an explanation of its colors has been offered by Rood (1860), Brücke (1864), and Aubert (1865). Aubert gives two patterns (*Phys. d. Netzhaut*, pp. 355, 378). Helmholtz also devotes some pages to the top, figuring two disc patterns (*Phys. Optik*, 1867, pp. 380, 381; new ed., pp. 530, 531). The demonstration of the subjective spectrum by its aid is a familiar experiment in psychophysical laboratories (*Cf.* Sanford's *Laboratory Course*, ch. vi).

The New Model Kinesimeter.

The following cuts illustrate the new model arm-rest and kinesimeter constructed at the Yale workshop for the Cornell Laboratory and described in the previous volume of the Journal.



New Model Arm-rest.



NEW MODEL KINESIMETER.

CONGRESSES, ETC.

The sixty-third annual meeting of the British Medical Association was held in London July 30 to August 2, 1895. Dr. Mickle was president, and Drs. McDowall, Nicolson, Rayner, Savage, Shaw and Weatherly vice-presidents of the psychological section. Discussions were held on the Treatment of Melancholia (introduced by Dr. Rayner), on Insanity in Relation to Criminal Responsibility (Dr. Maudsley), and on Epilepsy and its Relation to Insanity (Dr. Gowers). Papers were read by Dr. Bond on the "Relation of Diabetes to Insanity;" by Dr. Head on "Mental Symptoms in Relation to Bodily Diseases in the Sane;" by Dr. Maude on "Mental Symptoms in Relation to Exophthalmic Goitre;" by Dr. Reynolds on "Mental

Symptoms of Bodily Diseases;" by Dr. Shaw on "The Early Symptoms of Insanity;" by Dr. Campbell on "A Comparison of the Breaking-strain of Ribs in the Sane and the Insane;" by Dr. Percy Smith on "Voluntary Boarders in Asylums;" by Dr. Savage on "Insanity in Conduct;" by Dr. Shuttleworth on the "Operative Treatment of Idiocy;" and by Dr. Weatherly on the "Law in Relation to Single Patients."

The Late Professor Oliver.

The death of Professor J. E. Oliver of Cornell University should not pass unchronicled by psychological journals. Professor Oliver's interest in psychological questions is shown by his paper on "A Mathematical View of Free-will" (*Philosophical Review*, May, 1892), and by his participation in Miss Parrish's study of the cutaneous estimation of open and filled space (this JOURNAL, January, 1895). He also read the manuscript of Mr. Pillsbury's paper (published in the present number of the JOURNAL), having followed the investigation closely from its beginning.

Two of the problems which Professor Oliver had in mind for solution during the last two years of his life were psychological in nature. The first was that of the determination of the number of discriminable visual qualities (brightnesses and colors). He was keenly interested in the recent developments of optical theory, and especially in the question of the identity or difference of saturations and illuminations. The other—which was eminently characteristic of him—was that of attaching a mathematical or quasi-mathematical value to happiness. The ingredients of happiness were to be tabulated, by aid of the *questionnaire;* and these ingredients to be "weighted" in accordance with their statistical place in the whole series. The plan, if realized, would enable us to mark off the units upon what the Professor at the Breakfast Table calls "the dynamometer of happiness."

News from the Laboratories.

Professor Külpe, for many years Wundt's chief assistant at Leipzig, has been called to the chair of Philosophy at Würzburg. The University possesses at present no psychological laboratory.

Dr. E. Meumann, the author of important researches on the "time-sense" and on rhythm, will succeed Dr. Külpe at the Leipzig institute.

Professor Stumpf is busy furnishing a large laboratory at Berlin; and Professor Ebbinghaus will inaugurate one at Breslau. Dr. Schumann, who has been associated with Professor Müller at Göttingen, goes to Berlin, without habilitating, as Stumpf's assistant. Dr. Pilzecker (who has written on the attention under Müller's direction) succeeds Dr. Schumann.

M. V. Henri, one of the collaborators of M. Binet in the compilation of his *Introduction à la psychologie expérimentale*, is working for the Leipzig Doctorate with Wundt.

Dr. Marbe, the inventor of the new rotation apparatus, becomes assistant to Professor Martius at Bonn.

Professor Wundt is engaged upon a new edition of his *Logik*.